THURSDAY, SEPTEMBER 3, 1891.

THE REPORT OF THE BOARD OF TRADE COMMITTEE ON ELECTRICAL STANDARDS.

TARDILY, and in a somewhat piecemeal if not grudging fashion, some small provision has been made by Her Majesty's Government for the regulation, under the Board of Trade, of the new but vigorous and rapidly-extending industry which recent developments of electrical science have brought into existence. In no previously-existing branch of trade has the problem of settling standards of measurement been so difficult of solution, and in no other has the problem been so completely solved without trouble, expense, or intervention on the part of the Government itself. For the last twenty-five or thirty years a Committee of the British Association has laboured at the gigantic task of building up a system of units, which involved as a mere preliminary the revision of the conceptions and units of dynamics in order that these might form a basis for the definition of units for the far more complex physical quantities concerned in electricity and magnetism, quantities many of which had previously been by no means clearly apprehended, and which then received for the first time precise statement and definition.

Much of the work of the British Association Committee has been thankless, tedious, and, from its very nature, of a kind fitted to excite the cheap scorn of the self-styled "practical man," but it has made applied electricity possible, and has reacted in no slight degree on the progress of theory itself. The problem of the determination of the ohm—in other words, the process of realizing a standard of resistance according to the theoretical definition—has suggested problems to the theorist in the solution of which the theoretical investigator has been led to both direct and side-results of the very greatest value to the progress of science, and, in an unexpected manner, to the facilitation of practical applications. In no science have theory and practice been so closely connected during the last quarter of a century, and in none has the union been so markedly productive of good. By far the most interesting chapters of the history of electricity during the nineteenth century will be those that refer to its last three decades; may they chronicle a still closer alliance of the engineer and the experimenter, the electrical man of action and the mathematician! Here union is strength and dominion over the forces of Nature; disunion is waste of energy and slow progress in all that relates to the material, and therefore also to the social, advancement of the human race by means of electrical invention.

The establishment of the nucleus of an electrical standardizing laboratory in London, and the appointment towards the end of last year of a Committee to decide upon and recommend for adoption electrical standards for use in trade, testify to the great importance which the electrical industries have attained in this country in spite of the mistakes which attended their inception, and the general discouragement and disfavour with which they were received by the various interests they threatened.

The proceedings and report of the Committee have just been published in a blue-book, which contains matter of great interest to all engaged in electrical work. The vista which it opens up as regards the future operations of the standardizing laboratory may well dismay Her Majesty's Government; although no doubt due provision will ultimately be made for all its work. But of this at another time; at present we wish to direct attention to the resolutions of the Committee, which will be found in another page.

In the first place the Committee signify their adherence to the units of length, mass, and time as fundamental units, and adopt the C.G.S. system. This was only to be expected, for, after all, though some people may think that a better system could be devised if the work had to be done afresh, and they had a share in it, still collectively the body of scientific opinion is distinctly conservative, and there is little danger that any ill-advised attempt to disarrange the accepted system of theoretical and practical units will succeed.

Their third resolution, that the standard of electrical resistance should be called the ohm and should have the value 1,000,000,000 in terms of the centimetre and second in the ordinary electromagnetic system, is of great importance. It seems to settle once for all the question which has been debated over and over again, whether after a standard ohm has been realized, it will, like the standard yard or metre, be ever after the standard; or whether, if in case of variations in the physical properties of the substance, it shows an unexpectedly large divergence from the definition, a new standard ought to be constructed. Those who have assumed the former alternative have forgotten that the ohm is a derived unit, depending on the already fixed units of length, mass, and time, and that, therefore, its derivation ought to be as exact as the ever-widening resources of science can make it. For practical purposes of trade the standard fixed upon now and its copies are likely to remain undisturbed for a long time, and will probably only be corrected if there is serious alteration with time in their resistances. But the ohm will still be defined as 10^9 C.G.S. in the ordinary electromagnetic system of measurement, in which the magnetic permeability of air is assumed to be unity.

The fourth and fifth resolutions provide the definition of a practical realized ohm (1) by means of a column of mercury, (2) by comparison with the British Association unit, which it is stated may be taken as ·9866 of the ohm.

The wording of Resolution 4 strikes one as curious. The mercury column is to have a "constant cross-sectional area of 1 square millimetre." If "constant" has its ordinary sense of invariableness with time, the specification of 1 square millimetre renders it unnecessary. It has here apparently the usual sense of "uniform," that is, the section is the same at every part of the tube.

We are glad to see that the length adopted for the tube is 106·3 centimetres, instead of 106 centimetres, the round number adopted at the Paris Conference, and proposed, by the British Association Committee in 1886, to be legalized for a period of ten years. All the latest and best determinations of the ohm point to 106·3 as a convenient number very closely agreeing with the true value,

and its adoption now is probably only an anticipation of the decision which will be arrived at in a few years when the resolutions of that Conference are reconsidered.

In the adoption of a metallic working standard (announced in Resolution 5) the Committee only endorse an opinion long ago expressed by working electricians, that the mercury standards constructed in straight or spiral glass tubes are not practical instruments; they are difficult to handle, liable to breakage, and the only argument for their retention, the possible variability of metallic standards, has been shown to be almost baseless by the results of the continued and careful observation of the various metallic resistance coils deposited at Cambridge.

Passing over the resolutions which provide for copies, and multiples and submultiples of the ohm, with the remark that the long-felt want of trustworthy standards of low resistance will now at last be supplied, we come to the definition of the unit of current. Here again a theoretical definition corresponding to that of the ohm is given first; then for practical purposes it is stated "that an unvarying current which, when passed through a solution of nitrate of silver in water, in accordance with the specification attached to this report, deposits silver at the rate of 0·001118 of a gramme per second, may be taken as a current of 1 ampere." This is the most reasonable course that could have been adopted. The specification is practically one of the procedure adopted by Lord Rayleigh in his experiments on the electro-chemical equivalent of silver, and as Lord Rayleigh's absolute result was to be made the practical standard, it was right to recommend the same mode of experimenting.

Resolution 11, which defines the ampere in the case of an alternating current, was the subject of a good deal of discussion, and of some adverse comment by one of the witnesses examined on behalf of the electrical trades. The resolution states "that an alternating current of 1 ampere shall mean a current such that the square root of of the time-average of the square of its strength at each instant in amperes is unity." It was objected by the witness referred to, and by at least one member of the Committee, that this was giving a very special meaning to the term, which was inconsistent with the obvious definition, that of the simple time-average of the current. This latter average would, in the case of most periodic machines, be simply zero, unless the currents in the alternate half-periods were commutated so as to agree in sign with those in the other halves. But in the case of such a machine as the Brush, used for lighting incandescent lamps, the definition given in the resolution would have to be used; whereas if the machine were used for electro-plating, the simple time-average would have to be employed. This would give for the same current passing through the machine, from instant to instant, two different average values. The electric lighting application of periodic machines is, however, by far the most important, and the Committee did well, perhaps, to retain what is already the generally understood sense of the word *ampere* in connection with alternating currents. It ought to be, however, clearly understood that the main application of the definition will be to the measurements of currents in electric lighting, and that generally in other cases another definition will have to be employed.

Another important discussion took place over the definition of the standard unit of "pressure." In the first place, we should like to say here that we object entirely to the use of the term "pressure" in this connection. It has come as a sort of analogue of hydraulic pressure, and it has certainly led to very erroneous notions in the minds of the general public as to the functions of electric supply-mains, and also as to electricity itself. It is a pity that so many of the present pioneers of electricity, who are also leaders of physical science, should have countenanced by their example this misuse of a scientific term. We all know how strenuously some of these gentlemen have objected to the term "tension" as in "high-tension electricity"; surely "high-pressure instruments" and "electricity supplied at high pressure" are as objectionable, if not even more misleading. The use of the term *voltage*, or some such word, in the present Report, would have avoided the endorsement which it seems to give to what we think is a most unfortunate name for a physical quantity which is not a pressure at all; and it is to be hoped that the British Association Committee (who, by the way, were represented on the Committee of the Board of Trade) may be able to prevent this phrase from being added to the many other, though generally less objectionable terms which infest the literature of electricity.

A discussion arose as to whether the definition of the volt as the "pressure which, if steadily applied to a conductor whose resistance is one ohm, will produce a current of one ampere," was sufficiently definite. There might, it was argued, be an internal electromotive force in the conductor, and the "pressure" applied to the conductor might be regarded as that applied from the outside, or actually existent between its terminals, as shown by an electrometer. For example, the conductor might be the armature of a dynamo; the difference of potential might be considerable and the resistance only a small fraction of an ohm. In such a case it is, of course, well known that the electromotive force producing the current through any part of the armature resistance, according to Ohm's law, is the total internal electromotive force of that part, *minus* the difference of potential existing between its terminals, and it is this difference that is to be regarded as the applied "pressure" of the definition. In the same way in a voltameter, the electromotive force causing the current, according to Ohm's law, would be the existent or applied difference of potential, *minus* the internal back electromotive force developed by the chemical action. There were other difficulties about the specification of the ends of the conductor and the canalization of the current, and it was therefore thought desirable to adhere to the simple form of definition given in the report. It must be admitted that the definition leaves room for legal disputes in practice, and we think that it would have been perhaps better to have introduced on these points some kind of note or specification referred to in the resolution, so as to be taken along with it in the event of any dispute about the meaning of the definition.

A further question arose as to the provision of a practical standard of electromotive force in the form of a constant cell; and it was decided, partly in deference to the expressed wish of practical electricians, that the Clark cell should be adopted for this purpose. Its electromotive force, within certain limits of error to be deter-

mined by a sub-committee appointed for the purpose of preparing a specification for the construction and use of the cell, is stated to be 1·433 volts at the temperature 62° F. By means of this cell and known resistances, it will be possible to calibrate instruments without the use of electrolysis, and this to many persons would be the readiest and most easily carried out method. Of course, logically speaking, the standard of electromotive force is settled when those of resistance and current are fixed, and thus, if the order of definition is adhered to, the cell does not come in. But its electromotive force having been determined by careful measurement, and found to be so constant as it is, and so consistently the same in different specimens when the mode of construction is carefully attended to, it is too valuable a standard of reference to be set aside.

A very interesting discussion took place as to the mode of preparing these cells, and on the experience of different investigators as to their behaviour. Some of the divergences stated in the discussion were probably due to the different degrees of manipulative skill possessed by the various observers. A few careful experiments with different batches of cells carried out personally by the members of the committee interested in the matter would set the question at rest, and probably entirely confirm Lord Rayleigh's marvellously consistent results.

A side-point which came out in discussion is worthy of notice. We have not in this country any legal definition of temperature, whether Centigrade or Fahrenheit. In the definition of the standard yard 62° Fahrenheit is specified, but there is nothing to tell how that temperature is to be determined. It is well known (though apparently not to some of the text-book writers on heat) that mercurial thermometers, made with different kinds of glass, while agreeing at the freezing and boiling points, agree nowhere else, and all differ more or less from the air-thermometer. In very accurate work these discrepancies become very important, and thermometers must be calibrated by means of standards, if their indications are to be of any use for comparison. Some legal definition of temperature will, ere long, have to be given, and it seems rather a pity that the Committee did not practically settle this by saying what they meant by 62° Fahrenheit.

The definition of the volt for alternating currents, embodied in Resolution 15, is, of course, a mere consequence of Resolution 11, and these two definitions taken together are specially applicable to the measurement of the power spent in lighting incandescent lamps.

We have only to note that the Committee, in Resolutions 12 and 16, adopted instruments on the principle of the balance for the measurement of currents, and on the principle of Sir William Thomson's quadrant electrometer, used idiostatically, for the measurement of differences of potential, except for large differences, when an electrometer on the principle of the balance is to be employed. Thus the beautiful electrometers invented long ago by Sir William Thomson are likely to become at last, in a modified form, Board of Trade standards of exact measurement in industrial electricity. This is by no means the only striking example which could be cited of the thoroughly practical, because thoroughly theoretical, character of the instruments invented by one who understands all sides of the difficult problem involved in the invention and construction of such apparatus.

No resolutions were framed by the Committee on the very important subject of the measurement of power and energy. This must, however, come to the front before very long, and will tax the resources of the standardizing laboratory and its officials, assisted, as no doubt they will be, by Committees such as this which has just reported. We congratulate the Committee on the results of its labours, and trust that the requisite Order in Council will be passed before long confirming its resolutions. The laboratory will then be able to get to work, the necessary standards which have been asked for so long will be made accessible to those engaged in the electrical industries, and some serious difficulties under which they have laboured, in supplying electric light and power to the public, will be at last removed.

THE CONGRESS OF HYGIENE.

We print to-day a report of the important discussion in Section II. (Bacteriology) of the Congress of Hygiene, on "Immunity, Natural and Acquired":—

Dr. Roux, of the Institut Pasteur, in an introductory address, indicated the scope of the discussion. He began by saying that, in inviting a pupil of M. Pasteur to open the discussion on this subject, the Organizing Committee had reminded the Section that the great amount of interesting work which had recently been done on the subject had one point in common—namely, the attenuation of virus, and preventive inoculation, the two subjects with which M. Pasteur's name would for all time be honourably associated. With the single notable exception of vaccination, the only way of conferring immunity against any disease was the inoculation of the virus of the disease. To the old dangerous method of producing immunity by inoculation, Pasteur had added the less dangerous one of preventive inoculation by means of an attenuated virus, to which he had applied the term vaccination. The designation "attenuated" virus ought to be reserved for virus weakened without being attenuated —for example, by artificially lowering the vitality of the organisms for producing it.

Methods of Attenuation.—Two methods of attenuation had been described by M. Pasteur—namely, the prolonged exposure of a culture to air at a suitable temperature, and the passage of the micro-organisms through the bodies of different species of animals. Other methods had also been employed—for example, the action of heat, the use of antiseptics, of compressed oxygen and light.

In all cases, whatever the method employed, it was found to be necessary that the attenuation should be effected slowly and gradually; rapid attenuation rendered a virus altogether inactive without impressing on it any hereditary weakness. In whatever way the virus was prepared, it must, in order to confer immunity, be brought into direct contact with the tissues of the animal. In the early experiments the virus employed was always living; the living microbe, itself attenuated as to its virulence, was used. Another possible method of conferring immunity was the inoculation of the chemical substances produced by the micro-organisms.

Phagocytosis.—Dr. Roux next dealt with the doctrine of phagocytosis associated with the name of Dr. Metchnikoff. This observer had proved, by the study of the amœboid movement of certain cells that they possessed the power of including other cells and bodies in their substance. The phagocyte cells originated in the mesoderm. They possessed, further, the property of being able to digest the bodies which they had ingested. They were, in fact, the only cells which manifested in the human body any intracellular digestion. If the history of a bacterium in the interior of a phagocyte were followed, it would be seen that it underwent a peculiar series of alterations, very different from what took place when a microbe died in cultivating fluids. Whether a virulent virus was introduced into the bodies of animals which resisted inoculation, or whether attenuated microbes were injected

into sensitive animals, the greater the degree of refractoriness shown by the animal, the more rapidly the microbes were consumed by the leucocytes. In a non-resistant animal the microbes remained free; no such phenomenon as phagocytosis could be observed. It seemed, therefore, that the phagocytes were charged with the defence of the human organism, and entered into conflict with the parasites which infected the human frame. It might be said that there were diseases in which the microbes were to be met with in the cells specially, and that these microbes nevertheless proved fatal to the animal. In tuberculosis and in leprosy the bacilli were to be found in the cells, and the results were of the most serious kind, in spite of the intense phagocytosis induced by the microbes of these diseases. This fact proved that the phagocytes and all the other means of defence were, under certain conditions, and at certain times, powerless to effect any good results; they had done their best to take up the microbes, but these had adapted themselves to the interior of the cells, and had conquered. It was not sufficient that the microbes should be eaten up, it was essential that they should also be digested by the phagocytes. Even in those cases where the struggle was going against the human organism, these cells still were the aggressors. It had been frequently observed in tuberculosis and leprosy that the bacilli had been killed in the interior of certain of these cells. The theory asserted that a struggle occurred between the microbes and the cells, but it did not imply that the bacilli always won the day. Phagocytosis only occurred in immune animals; in animals susceptible to the disease it was either not to be observed, or it was incomplete.

He then proceeded to discuss the questions whether immunity was the consequence of this power of the cells to digest the virulent microbes. As had been said, the cells of a refractory animal took up the microbes, which, it would appear, under favourable circumstances remained inert in the interior of the cells. Numerous facts had been alleged to show that the microbes at the time they were taken up by the phagocytes were not degenerated, but were, on the contrary, in a condition of full activity. Thus, to take only one example, it had been found that in frogs the bacilli which had been taken up by the lecocytes remained alive within the protoplasm of the cell; this was apparent from their movements. In lymph taken from the body of a pigeon, numerous bacilli were to be seen imprisoned in the leucocytes, and these bacilli could be watched growing, actually under the eye of the observer, within the interior of dead phagocytes; they could be seen to elongate, to push out the protoplasm, distort the form of the cell, and finally to make their escape. Another demonstration of the importance of the action of the phagocytes was afforded by the fact that even in immune animals the microbes were found to increase when kept out of the reach of the leucocytes; thus, if a rabbit were inoculated in the anterior chamber of the eye, where there were no cells, the bacteria grew freely, and their development was only checked when the leucocytes had after a time migrated in large numbers, and began to take the microbes into their interior. It thus appeared that phagocytosis was a very general phenomenon, and one which was very efficacious in checking the advance of the organisms; when it failed, the individual succumbed to the virulence of the bacteria. The question remained, What was the mysterious force which attracted the cells towards the microbes? Why were the leucocytes, which in immune animals destroyed the microbes, incapable of seizing upon them in non-immune animals?

In 1883, Metchnikoff propounded his theory of phagocytosis. This theory rested on two assumptions: first, that the cells were attracted to the microbes in virtue of a special sensibility manifested towards all foreign bodies introduced into the tissues; the second was that this power of seizing upon the virulent microbes in immune animals originated in a habit formed during the earlier struggle with the attenuated virus with which the animal had been previously inoculated. The behaviour of the leucocytes might be more readily explained by assuming that leucocytes had the property, analogous to that possessed by the zoosperms of the myxomycetes—namely, that of being attracted by certain bodies and repelled by others. MM. Massart and Bordet had proved that the products of the microbes exerted a very marked chemical action on the phagocytes. When a virus was introduced into the body, it proliferated, and secreted a substance which attracted the leucocytes; the more active the virus, the more energetic were the poisons elaborated by it, and the cells which penetrated to the point of inoculation were paralyzed in their action, and rendered incapable of taking up the microbes, which therefore proliferated without hindrance. Further, in certain diseases the virus produced a substance which was still more poisonous. In chicken cholera, for instance, the poison secreted by the microbes repelled the leucocytes from the point of inoculation; it thus came about that phagocytes were never found in this particular affection. This, however, was not the case with animals which had been rendered immune either by inoculation of the attenuated virus, or by the injection of a suitable dose of bacterial products. If the animal were given a strong virus, phagocytes were attracted to the point of inoculation, and these possessed the power of taking up the microbes before they had time to elaborate effective doses of their toxic material. It was, therefore, at the commencement of the disease that the critical struggle took place. If the leucocytes could not accomplish this at the beginning of the malady, their action at a later period would be useless, since the microbes would have produced enough poison to paralyze their activity. Every cause, therefore, that prevented the access of leucocytes to the point of inoculation facilitated infection. The theory of immunity propounded by M. Metchnikoff did not exclude the possibility of there being other means of protecting the organism, but it simply proved that phagocytosis had a wider sphere of action, and was more efficacious, than any other means of protecting the organism. It seemed to explain all the facts, and was, moreover, eminently suggestive. It was in this way that the knowledge of microbic poisons and chemical inoculation had thrown light on what would otherwise have been obscure. Far from being shaken by the theories which were opposed to it, this theory of Metchnikoff's had gained by the opposition which it has met, and that was a guarantee of its soundness.

Dr. Buchner, of Munich, after giving a general account of the various theories of immunity, criticized freely Metchnikoff's views. The main objections he brought forward were as follows:—

(1) Many observers failed to notice any destruction of bacilli by phagocytes, when naturally immune animals, such as white rats or pigeons, were inoculated with anthrax.

(2) In diseases ending fatally, such as tuberculosis, mice-septicæmia, &c., the micro-organisms were frequently found in the interior of phagocytes.

(3) The experiments of Petruchky, Baumgarten, Pekelharing, and others seemed to show that the bacilli of anthrax perished in the living fluids of immune animals even when the bacilli were protected against the attacks of white corpuscles.

Metchnikoff, however, denied this, and proved that the living fluids of immune white rats form a most excellent cultivating medium for the bacilli of anthrax. These observations of Metchnikoff, according to Buchner, might be explained by the fact that Metchnikoff in his experiments introduced more bacilli than could be destroyed by the living fluids of white rats, as a certain quantity of serum was able to destroy only a very small quantity of micro-organisms. Speaking of the experiments made by his pupils Ibener and Roeder, he stated that, when a certain kind of micro-organisms were placed into a given quantity of serum, the micro-organisms might either be destroyed *in toto*, or reproduce themselves in large numbers according to the number of micro-organisms introduced in the first place into the serum. When, instead of placing the micro-organisms directly in contact with the serum, the micro-organisms were wrapped up in sterilised cotton-wool, it was found that the bacilli, so protected against the temporary harmful influence of serum, began to grow luxuriantly at the end of twenty-four hours. The bactericidal power of serum disappeared, therefore, shortly after death.

Massart, Bordet, and Gabritchewsky had previously proved that the emigration of leucocytes to the spot where the virus was introduced was due to the attracting influence (positive chemotaxis) of the chemical poisons secreted by micro-organisms, but he (Buchner) was of opinion that the substances dissolved in the cultures have hardly any action on leucocytes, but that this attracting influence on leucocytes was due to the protein present in bacterial cells themselves. Whereas the products of the metabolism of micro-organisms had little or no attracting influence on the leucocytes, the proteins themselves attracted the cells most powerfully.

As long as the bacterial cells were active and capable of reproducing themselves actively, the proteins were contained in the cells, and these poisons only left the cells when the latter

became diseased or old. Hence these proteins were chiefly found in old cultures, the filtered and sterilised extracts of which always possessed a strong attracting influence on leucocytes. Hence it followed that, "The more a given micro-organism is harmfully influenced by the living fluids of a given species of animals, the more proteins will be excreted. This, as a natural consequence, is followed by a corresponding increase in the number of cells which emigrate to the point of inoculation." In every case the living fluids of the body exert a harmful influence on micro-organisms, and then, when in consequence of this the excretion of proteins takes place, the amœboid cells emigrate to the spot.

Turning now to the characteristics of this germicidal substance present in serum, he thought that this germicidal power gradually disappeared, so that after a few days the serum had no bactericidal power. This germicidal action was destroyed by the micro-organisms themselves, for, unless the latter were completely destroyed, they soon began to grow freely in serum. This germicidal substance was easily destroyed by heat. Serum which had been maintained at 55° C. during half an hour, or at 52° C. during six hours, lost its bactericidal power completely. A moderate degree of warmth (37° C.) intensified the germicidal action of the blood or serum.

Turning now to the question as to whether this bactericidal action of the blood had any share in the production of immunity, he gave the following facts as proving that there was some connection between the immunity of a given animal against a given infectious disease, and the bactericidal action of its blood on the micro-organism producing the disease:—

(*a*) The blood and serum of animals, such as mice and guinea-pigs, which readily succumbed to anthrax had no bactericidal power on anthrax-bacilli.

(*b*) The serum of animals which took anthrax readily never possessed such a strong bactericidal action as the serum of white rats, which were immune against anthrax.

(*c*) The blood and serum of animals rendered artificially immune possessed stronger bactericidal powers than the blood and serum of normal animals.

(*d*) The blood and serum of animals rendered artificially immune against a given micro-organism lessened the virulence of the specific micro-organism causing the disease.

(*e*) Whenever blood and serum possessed no bactericidal action on micro-organisms, this absence of bactericidal action might be due to the fact that, owing to the necessary manipulations, this bactericidal substance had been altered or even destroyed.

As further proving that the immunity of animals depended on some substance present in the serum, he mentioned the facts described by Behring, Kitasato, Ogata, and Emmerich, in which the injection of blood or serum of an animal immune against a given bacillus, cured another animal afflicted with the same disease. This curative power he attributed to the presence in the blood of immune animals of a protective substance, probably proteid in its nature, to which he gave the name of "alexine" (from ἀλεξεῖν, to protect). These alexines were not ordinary oxidation products of the tissues, as they were quite specific in their action. They were not simply enzymes, as they had no hydrolytic properties, but they were most probably proteid substances. These alexines were probably formed in the cells; but, when formed, their action was quite independent from that of cells, and they were probably always present in immune animals.

Mr. E. H. Hankin, of Cambridge, after giving a *résumé* of the work done by various observers, said that theoretical considerations led him to suspect that a particular ferment-like proteid, known as cell globulin B, was a substance possessing bactericidal power. He tested its action on anthrax bacilli, and found that it had the power of destroying these microbes.

He further found that similar substances were present, not only in animals that were naturally immune against anthrax, but also in those that were susceptible to this disease. To these substances he had given the name of *defensive proteids*. In his published papers on this subject he had noted various similarities in the bactericidal action of these substances, and that possessed by blood-serum, and these resemblances were such as to leave little room for doubt that the bactericidal action of blood-serum was due to the presence of these defensive proteids.

The serum of white rats contained a proteid body possessing a well-marked alkaline reaction, and a power of destroying anthrax bacilli. Further, when injected into mice along with fully virulent anthrax spores, it would prevent the development of the disease. On the other hand, defensive proteids of animals susceptible to anthrax did not exert such protective power, and consequently these experiments indicated a difference in the mode of action of defensive proteids of immune and non-immune animals respectively. Further, the amount of defensive proteid present in a rat could be diminished by the causes which were known to be capable of lowering the animal's power of resisting anthrax. For instance, Feser stated that rats become susceptible to anthrax when fed on a vegetarian diet. Mr. Hankin obtained similar results with wild rats. The ordinary white rat he found to be generally refractory to anthrax on any diet, and the defensive proteid could always be obtained from its spleen and blood-serum. This was not the case with wild rats. In one experiment eight wild rats were used; of these, four were fed on bread and meat, the others on plain bread, for about six weeks. Then one rat of each lot was inoculated with anthrax; of these, the one that had been subjected to a bread diet succumbed. The remaining rats were killed, and it was found that while the spleens of the flesh-fed rats contained abundance of the defensive proteid, only traces of this substance could be obtained from the spleens of the rats that had been fed on bread alone. A similar result was obtained in other experiments.

Very young rats were known to be susceptible to anthrax, and so far as could be judged from the litmus test (after dialysis and addition of NaCl), their serum appeared to contain less of the defensive proteid than did that of the adult rat. Further, Mr. Hankin found that a young rat could be preserved from anthrax by an injection of its parent's blood-serum.

These facts appeared to prove that the defensive proteid of the rat deserved its name, in that it preserves the animal from the attack of the anthrax microbe; in other words, that this substance was at any rate a part cause of the rat's immunity against anthrax.

Defensive proteids appeared to be ferment like, albuminous bodies, and it was extremely unlikely that we should for a considerable time be able to classify them by any other than physiological tests. From this point of view it was possible to divide them into two classes; first, those occurring naturally in normal animals, and secondly, those occurring in animals that have artificially been made immune. For these two classes Mr. Hankin proposed the names of *sozins* and *phylaxins*. A "sozin" was a defensive proteid that occurred naturally in a normal animal. They had been found in all animals yet examined, and appear to act on numerous kinds of microbes or on their products. A "phylaxin" was a defensive proteid which was only found in an animal that had been artificially made immune against a disease, and which (so far as is yet known) only acted on one kind of microbe or on its products.

Each of these classes of defensive proteids could obviously be further subdivided into those that acted on the microbe itself, and those that acted on the poisons it generated. These sub-classes he proposed to denote by adding the prefixes myco- and toxo- to the class name. Thus myco-sozins were defensive proteids occurring in the normal animal, which had the power of acting on various species of microbe. Toxo-sozins were defensive proteids, also occurring in the normal animal, having the power of destroying poisons produced by various microbes. Myco-phylaxins and toxo-phylaxins similarly would denote the two sub-classes of the phylaxin group.

The classification might be represented by the following scheme:—

Defensive proteids (Hankin). Alexins (Buchner).	Sozins:— Defensive proteids present in the normal animal.	Myco-sozins:— Alkaline globulins from rat (Hankin), destroying anthrax bacillus.
		Toxo-sozins:— Of rabbit, destroying *V. metchnikovi* poison (Gamaleia).
	Phylaxins:— Defensive proteids present in the animal after it has been made artificially immune.	Myco-phylaxins:— Of rabbit, destroying pig typhoid bacillus (Emmerich).
		Toxo-phylaxins:— Of rabbit, &c., destroying diphtheria and tetanus poisons (Behring and Kitasato, antitoxin of Tizzoni and Cattani).

Prof. Emmerich, of Munich, read a paper on "The Artificial Production of Immunity against Croupous Pneumonia and the

Cure of this Disease." He stated that his previous experiments on swine fever had proved that in immune animals the bacilli of swine fever were destroyed, not by the cells of the animal, but by a bactericidal substance present in the blood. It had been clearly proved by his experiments that the bacilli of swine fever were destroyed almost immediately after their introduction under an immune animal's skin. Applying these researches to the disease produced in rabbits by the inoculation of the *Diplococcus pneumoniæ* of Fraenkel, he showed that non-immune rabbits died within twenty-four to forty-eight hours after the introduction of the virus. But if such animals had been previously treated with the blood or serum of animals rendered artificially immune against the diplococcus of Fraenkel, such animals did not die, but recovered after the introduction of extremely virulent diplococci. Moreover, when the *Diplococcus pneumoniæ* was inoculated into an animal, it was possible to cure it by injecting shortly afterwards some of the serum of an animal rendered artificially immune. In the blood of animals rendered artificially immune against pneumonia we possessed an excellent cure for the disease. Not only would it be possible to cure men afflicted with pneumonia by these injections, but we could, by preventive inoculations applied in time, put a stop to the spread of an epidemic in a school or a prison for instance. His experiments, together with Dr. Doenissen's, had a great practical as well as a theoretical value.

Dr. Ehrlich, of Berlin, stated that he had lately made a number of experiments with ricin which threw great light on the question of immunity. According to Kobert and Stillmark, ricin was an extremely poisonous body, for it acted fatally when such small doses as 0·03 mg. were injected into an animal's veins. When absorbed through the alimentary canal, a dose 100 times larger could be easily tolerated. Nevertheless, even then, it was so toxic that, according to Kobert's reckoning, a dose of 0·18 gr. would prove fatal to a full-grown man. It had a harmful influence on the blood, producing coagulation of the red blood-corpuscles, and thromboses, more especially of the vessels of the alimentary canal.

In his opinion the toxicity of ricin greatly depended on the species of animals used for experiments, the animals most susceptible to its action being guinea-pigs. Thus, a guinea-pig weighing 385 grammes died eleven days after the inoculation of 0·7 cc. of a 1 in 150,000 solution of ricin, the *post-mortem* examination showing characteristic hæmorrhages in the alimentary tract. One gramme of this substance might therefore prove fatal to 1,500,000 guinea-pigs. White mice, on the other hand, did not die after much larger doses, and this immunity of mice against this poison might be increased by subcutaneous injections of ricin. The same result might be obtained, however, far more easily and without any chances of failure, by feeding mice with ricin. It was best to begin with small, harmless doses, gradually increasing the amount until the organism was accustomed to the poisonous substance. In ten days a mouse might then be inoculated with a deadly or even larger dose without suffering any evil effects. Thus, whilst doses of 1/200000 gramme was absolutely fatal in normal animals, mice fed daily and in increasing quantities with ricin suffered no harm after the injection of 1/1000 gr. or 1/500 gr., or, occasionally, of 1/250 gr.

Whilst a 0·5 or 1 per cent. solution of ricin applied to the eye of a normal animal produced severe inflammation and panophthalmitis, the application of a 10 per cent. solution of ricin produced no effect on the eye of an animal previously fed with ricin. In other words, this was distinct proof of the existence of a local as well as of a general immunity against the poison. Strangely enough it was almost impossible to render the subcutaneous tissue immune against ricin, and even in exceedingly immune animals the subcutaneous injection of ricin produced distinct necrosis of the subcutaneous tissue.

It was a remarkable fact that this immunity appeared quite suddenly on the sixth day, and then increased slowly, so that on the twenty-first day the animal could stand a dose which was 400 times higher than that fatal to a normal animal.

This immunity against ricin appeared to be permanent, for it was still present in immune mice which had not taken ricin for a period of six months previously.

He had been able to extract from the blood of animals rendered immune against ricin a body which had the power of counteracting the toxic action of ricin, so that a powerful solution of ricin was rendered harmless by admixture with the blood of immune mice. It was also possible to render animals immune against ricin by injecting the blood of immune animals.

He had obtained similar results with abrin, which would be shortly published.

Dr. Kitasato, of Tokio, shortly summarized the results which he and Dr. Behring had obtained with the virus of tetanus. According to these observers, the blood of a normal rabbit has no influence on the toxines secreted by the bacillus of tetanus. But when a rabbit had been rendered artificially immune against that disease, its blood had the power of destroying the toxines secreted by the specific bacillus. Nay, more, the blood of rabbits made artificially immune against tetanus with trichloride of iodine, rendered mice not only refractory to tetanus but also cured the disease when already in progress. The blood, however, did not appear to act on the tetanus bacillus itself, but on the toxines secreted by the bacillus.

Dr. Adami, of Cambridge, thought that it was impossible to doubt that in a large number of infectious diseases the process of phagocytosis was extremely marked. He was of opinion that it was quite possible to accept both views of the question. The controversy had taken place chiefly as to the phenomena observed in the rat; in that animal phagocytosis was only to be observed with difficulty, and the serum of rat's blood undoubtedly possessed bacteria-killing properties to a high degree.

Dr. Klein, of London, stated that frogs and rats were insusceptible to anthrax, but that these animals could be made susceptible to the disease by a variety of means, indicating that their normal power of resistance was due to certain chemical conditions of the blood. If the bacillus of anthrax was introduced into the lymph-sac of a chloroformed frog, this animal always died of anthrax. Rats inoculated with anthrax and kept under the influence of an anæsthetic also died of anthrax. He had been unable to find any evidence to show that in these cases the leucocytes had lost their power of swallowing up bacteria, and therefore the susceptibility of chloroformed animals to anthrax could only be explained by some chemical changes taking place in the serum of the chloroformed rat or frog.

Dr. Metchnikoff, of Paris, who was greeted with loud and prolonged cheering, said that, of all the objections which have been raised against the theory of phagocytes, doubtless by far the most important was that formulated by Behring and Nissen: namely, the fact that the serum of guinea-pigs vaccinated against the vibrio of Metchnikoff had bactericidal powers on the same vibrio. Whilst the serum of normal guinea-pigs allowed the free development of a large number of these microbes, the serum of vaccinated animals killed the micro-organisms at the end of a few hours. MM. Behring and Nissen were convinced that this fact formed a complete explanation of the acquired immunity of guinea-pigs against the *Vibrio Metchnikofi*, and that it might serve as a model for a theory of immunity. His own researches, however, proved the contrary. If one studied the phenomena as they occurred in the living animal, one noticed at once that the bacilli inoculated into immune guinea-pigs remained alive for a very long time. Some vibrios were taken into the interior of leucocytes at the point of inoculation, whilst others developed perfectly in the liquid exudation. To show this, one had only to take a drop of the latter, and place it in the warm chamber; the leucocytes perished when taken out of the organism, and allowed the bacilli contained in their interior to develop freely. The vibrions thus multiplied and filled the leucocytes, which swelled and eventually burst, allowing the microbes to pass freely into the liquid part of the exudation. Here the development continued, and one obtained very abundant cultures from the liquid exudation of the immune guinea-pig. If one extracted a small quantity of such a culture, and introduced it into the dead serum of an immune guinea-pig, this serum not only did not kill the bacilli, but also gave a more abundant development than the serum of a non-immune animal could do. The study of the phenomena in living animals made artificially immune against the vibrio of Metchnikoff, instead of overthrowing the theory of phagocytosis, furnished on the contrary an evident proof in its favour. The theories of the attenuation of virus in the bodies of immune animals, and of the neutralization of the toxines, could not be applied to his case, as the vibrios remained very virulent, and because the immune guinea-pigs are as sensitive to the toxine of the bacillus as the non-immune animal.

This example showed yet once more that one must not be content with studying the phenomena of immunity outside the

organism. This criticism also applied to M. Buchner's experiments, which he had communicated to this meeting; he insisted on the fact that, in order to assure one's self thoroughly of the bactericidal property of the serum, it was necessary to take a small quantity of the culture, and spread it in a tube filled with serum. If, according to Dr. Buchner, one introduced a little of the culture wrapped in cotton-wool, the serum could no longer exercise its bactericidal power, and the microbe developed freely. Now, when one inoculated the bacillus under the skin of an animal, one introduced at the same time a small mass which did not spread freely in the blood or exudation, but remained localized at one spot. The experiments of Mr. Buchner, instead of furnishing an objection to the phagocyte theory, rather supported it.

Referring to the curative properties of the serum of white rats against anthrax, he had come to the conclusion that, whereas the living serum of white rats had no bactericidal action on anthrax, the dead serum of the same animals had marked bactericidal powers on the same micro-organism. When a mouse was inoculated with a mixture of the dead serum of a rat and anthrax bacilli, it nearly always died, although the disease lasted somewhat longer than usual. On examination of the point of inoculation it was found that the bacilli of anthrax did not grow quite so readily, and that an enormous number of leucocytes emigrated to the point of inoculation and took the bacilli into their interior and digested them. In tetanus, again, the leucocytes ate up considerable quantities of tetanus-spores and bacilli. Summing up his researches, he stated that whenever an animal recovered from an infectious disease this recovery was accompanied by a process of phagocytosis; whenever an animal died of an infectious disease the process of phagocytosis was absent or insufficient. The theory of phagocytes was strictly based on the principles of evolution as laid down by Darwin and Wallace.

After some remarks by Dr. Fodor, Dr. Cartwright Wood, Prof. Babes, Dr. Wright, and Dr. Arloing,

Dr. Roux, answering some remarks made by Prof. Emmerich, stated that, far from the preventive inoculations against anthrax and swine fever having been proved to be unsuccessful, agriculturists in France and other countries were making use of them daily, and the u e of the various vaccins manufactured at the Institut Pasteur was increasing day by day.

Dr. Buchner congratulated Dr. Metchnikoff on his most important paper. He was of opinion, however, that the time for framing a complete theory of immunity had not come yet.

Sir Joseph Lister then stated that if anything were required to justify the existence of this Congress it would have been their sitting that day. The immense amount of valuable material which they had had on this most important subject had been such as to make all the members exceedingly grateful to those who had brought these matters before them.

THE BRITISH ASSOCIATION.

THE following is a list of the grants of money appropriated to scientific purposes by the General Committee at the Cardiff meeting, August 1891. The names of the members entitled to call on the General Treasurer for the respective grants are prefixed:

	£	s.	d.
A.—*Mathematics and Physics.*			
*Foster, Prof. Carey—Electrical Standards (partly renewed)	27	4	6
*McLaren, Lord—Meteorological Observations on Ben Nevis	50	0	0
*Symons, Mr. G. J.—Photographs of Meteorological Phenomena	15	0	0
*Cayley, Prof.—Pellian Equation Tables (partly renewed)	15	0	0
*Rayleigh, Lord—Tables of Mathematical Functions	15	0	0
*Fitzgerald, Prof. G. F.—Electrolysis	5	0	0
*Lodge, Prof.—Discharge of Electricity from Points	50	0	0
*Thomson, Sir W.—Seismological Phenomena of Japan	10	0	0
B.—*Chemistry and Mineralogy.*			
*Roberts-Austen, Prof.—Analysis of Iron and Steel (renewed)	8	16	0
*Armstrong, Prof. H. E.—Formation of Haloids from Pure Materials (partly renewed)	25	5	0
*Tilden, Prof. W. A.—Properties of Solutions ...	10	0	0
*Thorpe, Prof.—Action of Light upon Dyed Colours (partly renewed)	10	0	0
C.—*Geology.*			
*Prestwich, Prof.—Erratic Blocks (partly renewed)	15	0	0
*Wiltshire, Rev. T.—Fossil Phyllopoda (renewed)	10	0	0
*Geikie, Prof. J.—Photographs of Geological Interest	20	0	0
*Woodward, Dr. H.—Registration of Type Specimens of British Fossils (renewed)	5	0	0
*Hull, Prof. E.—Underground Waters	10	0	0
*Davis, Mr. J. W.—Investigation of Elbolton Cave	25	0	0
Jones, Prof. R.—Faunal Contents of *Sowerbyi* Zone	10	0	0
*Evans, Dr. J.—Excavations at Oldbury Hill ...	25	0	0
*Woodward, Dr. H.—Cretaceous Polyzoa... ...	10	0	0
D.—*Biology.*			
*Sclater, Dr. P. L.—Table at the Naples Zoological Station	100	0	0
*Lankester, Mr. E. R.—Table at Plymouth Biological Laboratory (renewed)	17	10	0
*Haddon, Prof. A. C.—Improving a Deep-sea Tow-net (partly renewed)	40	0	0
*Newton, Prof.—Fauna of Sandwich Islands (renewed)	100	0	0
*Sclater, Dr. P. L.—Zoology and Botany of the West India Islands (renewed)	100	0	0
E.—*Geography.*			
Ravenstein, Mr. E. G.—Climatology and Hydrography of Tropical Africa	75	0	0
H.—*Anthropology.*			
*Flower, Prof.—Anthropometric Laboratory ...	5	0	0
Garson, Dr. J. G.—Prehistoric Remains in Mashonaland	50	0	0
*Tylor, Dr. E. B.—North-western Tribes of Canada	100	0	0
*Turner, Sir W.—Habits, Customs, &c., of Natives of India (renewed)	10	0	0
*Flower, Prof.—New Edition of Anthropological Notes and Queries	20	0	0
*Symons, Mr. G. J.—Corresponding Societies' Committee...	25	0	0
	£1013	15	6

* Reappointed.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY E. G. RAVENSTEIN, F.R.G.S., F.S.S., PRESIDENT OF THE SECTION.

The Field of Geography.[1]

IT behoves every man from time to time to survey the field of his labours, and to render an account unto himself of the work he has accomplished, and of the tasks which still await him, in order that he may perceive whether the means employed hitherto are commensurate with the magnitude of his undertaking, and likely to lead up to the desired results. Such a survey of the "Field of Geography" I propose to make the subject of my address to-day. . . .

Whatever changes may have taken place respecting the aims of the geographer, it is very generally acknowledged that the portraiture of the earth's surface in the shape of a map lies within his proper and immediate domain. And there can be no doubt that a map possesses unique facilities for recording the fundamental facts of geographical knowledge, and that with a

[1] Pressure on our space compels us to omit some parts of this address.

clearness and perspicuity not attainable by any other method. You will not, therefore, think it strange if I deal at considerable length with the development of cartography, more especially as my own labours have in a large measure been devoted to that department of geographical work. An inspection of the interesting collection of maps of all ages which I am able to place before you will serve to illustrate what I am about to say on this subject. . . .

Ptolemy, like all great reformers, stood upon the shoulders of the men who had preceded him, for before a map like his could be produced much preliminary work had been accomplished. Parmenides of Elea (460 B.C.) had demonstrated that our earth was a globe, and Eratosthenes (276-196 B.C.) had approximately determined its size. Hipparchus (190-120), the greatest astronomer of antiquity, the discoverer of the precession of the equinoxes, and the author of a catalogue of stars, had transferred to our earth the auxiliary lines drawn by him across the heavens. He had taught cartographers to lay down places according to their latitude and longitude, and how to project a sphere upon a plane. It is to him we are indebted for the stereographic and orthographic projections of the sphere. Ptolemy himself invented the tangential conical projection.

The gnomon or sun-dial, an instrument known to the Chinese 600 years before Christ, had long been used for the determination of latitudes, and the results were relatively correct, although uniformly subject to an error of 16 minutes, which was due to the observers taking the altitude of the upper limb of the sun, when measuring the shadow cast by their dial, instead of that of the sun's centre.

It was known, likewise, that differences of longitude could be determined by the simultaneous observation of eclipses of the sun or moon, or of occultations of stars, and Hipparchus actually calculated ephemerides for six years in advance to facilitate computations. Ptolemy himself suggested the use of lunar distances. But so imperfect were the astrolabes and other instruments used by the ancient astronomers, and especially their time-keepers, that precise results are quite out of the question.

Ptolemy, in fact, contented himself with accepting eight latitudes determined by actual observation, of which four were in Egypt, whilst of the three longitudes known to him he only utilized one in the construction of his map. Unfortunately, the one selected proved the least accurate, being erroneous to the extent of 32 per cent., whilst the error of the two which he rejected did not exceed 13 per cent.[1] This want of judgment—pardonable, no doubt, under the circumstances—vitiated Ptolemy's delineation of the Mediterranean to a most deplorable extent, far more so than did his assumption that a degree only measured five hundred stades, when in reality it measures six hundred. For whilst the breadth of his Mediterranean, being dependent upon the relatively correct latitudes of Alexandria, Rhodes, Rome, and Massilia, fairly approximates the truth, its length is exaggerated to the extent of nearly 50 per cent., measuring 62° instead of 41° 40′. This capital error of Ptolemy is due therefore to the unfortunate acceptance of an incorrect longitude, quite as much as to an exaggeration of itinerary distances. It is probable that Ptolemy would have presented us with a fairer likeness of our great inland sea had he rejected observed latitudes and longitudes altogether, and trusted exclusively to itineraries and to such bearings as the mariners of the period could have supplied him with.

No copy of Ptolemy's original set of maps has reached us, for the maps drawn by Agathodæmon in the fifth century are, under the most favourable circumstances, merely reductions of Ptolemy's originals, or they are compiled from Ptolemy's "Geography," which, apart from a few explanatory chapters, consists almost wholly of lists of places, with their latitudes and longitudes. I am almost inclined to adopt the latter view—firstly, because of the very crude delineation of Egypt, for which country an accurate cadastral survey was available; and secondly, on account of the cylindrical projection on which these maps are drawn, although from Ptolemy's own statements we are justified in believing that he made use of a conical projection in the construction of his maps.

[1] The three longitudes are the following:—

	Result of ancient observations.	Adopted by Ptolemy.	Actual difference of longitude.
Arbela ...	45° E. of Carthage ...	45° ...	34°
Babylon ...	12° 30′ E. of Alexandria ...	18° 30′ ...	14° 18′
Rome ...	20° E. of Alexandria ...	23° 50′ ...	17° 24′

An examination of Ptolemy's maps shows very clearly that they were almost wholly compiled from itineraries, the greater number of which their author borrowed from his predecessor Marinus. It shows, too, that Ptolemy's critical acumen as a compiler cannot be rated very high, and that he failed to utilize much information of a geographical nature which was available in his day. His great merit consisted in having taught cartographers to construct their maps according to a scientific method. This lesson, however, they were slow to learn, and centuries elapsed before they once more advanced along the only correct path, which Ptolemy had been the first to tread.

During the "Dark Ages" which followed the dismemberment of the Roman Empire there was no lack of maps, but they were utterly worthless from a scientific point of view. The achievements of the ancients were ignored, and the principal aim of the map-makers of the period appears to have been to reconcile their handiwork with the orthodox interpretation of the Holy Scriptures. Hence those numerous "wheel maps," upon which Jerusalem is made to represent the hub, whilst the western half of the disk is assigned to Europe and Africa, and the eastern to Asia.

As it is not my intention to introduce you to the archæological curiosities of an uncritical age, but to give you some idea of the progress of cartography, I at once pass on to the Arabs.

The Arabs were great as travellers, greater still as astronomers, but contemptible as cartographers. Their astronomers, fully possessed of the knowledge of Ptolemy, discovered the error of the gnomon; they improved the instruments which they had inherited from the ancients, and carefully fixed the latitudes of quite a number of places. Zarkala, the Director of the Observatory of Toledo, even attempted to determine the difference of longitude between that place and Bagdad; and if his result differed to the extent of 3° from the truth, it nevertheless proved a great advance upon Ptolemy, whose map exhibits an error amounting to 18°. Had there existed a scientific cartographer among the Arabs, he would have been able, with the aid of these observations, and of the estimates of distances made by careful observers like Abul Hasan, to effect most material corrections in the map of the known world. If Edrisi's map (1154) is better than that of others of his Arab contemporaries, this is simply due to his residence at Palermo, where he was able to avail himself of the knowledge of the Italians.

Quite a new epoch in the history of cartography begins with the introduction of the magnetic needle into Europe. Hitherto the seaman had governed his course by the observation of the heavens; thenceforth an instrument was placed in his hands which made him independent of the state of the sky. The property of the magnet or "loadstone" to point to the north first became known in the eleventh century, and in the time of Alexander Neckam (1185) it was already poised upon a pivot. It was, however, only after Flavio Gioja of Amalfi (1302) had attached to it a compass-card, exhibiting the direction of the winds, that it became of such immediate importance to the mariner. It is only natural that the Italians, who were the foremost seamen of that age, should have been the first to avail themselves of this new help to navigation. At quite an early date, as early probably as the twelfth century, they made use of it for their maritime surveys, and in course of time they produced a series of charts upon which the coasts frequented by them, from the recesses of the Black Sea to the mouth of the Rhine, are delineated for the first time with suprising fidelity to nature. The appearance of these so-called compass-charts, with gaily coloured roses of the winds and a bewildering number of rhumb-lines, is quite unmistakable. A little consideration will show you that if the variation of the compass had been taken into account in the construction of these charts, they would actually have developed into a picture of the world on Mercator's projection. But to deny them all scientific value because they do not fulfil this condition, is going too far. As correct delineations of the contours of the land they were a great advance upon Ptolemy's maps, and it redounds little to the credit of the "learned" geographers of a later time that they rejected the information so laboriously collected and skilfully combined by the chart makers, and returned to the deformities of Ptolemy. The adjustment of these charts to positions ascertained by astronomical observations could have been easily effected. An inspection of my diagrams

will prove this to you. The delineation of Italy, on the so-called Catalan map, is surprisingly correct; whilst Gastaldo, whose map of Italy is nearly two hundred years younger, has not yet been able to emancipate himself from the overpowering authority of Ptolemy. And in this he did not sin alone, for Italian and other cartographers of a much later time still clung pertinaciously to the same error.

There were others, however, who recognized the value of these charts, and embodied them in maps of the entire world. Among such were Marino Sanuto (1320) and Fra Mauro (1453), both of whom made their maps the repository of much information gathered from the Arabs or from their own countrymen who had seen foreign parts. Fra Mauro, more especially has transmitted to us a picture of Abyssinia marvellously correct in its details, though grossly exaggerated in its dimensions.

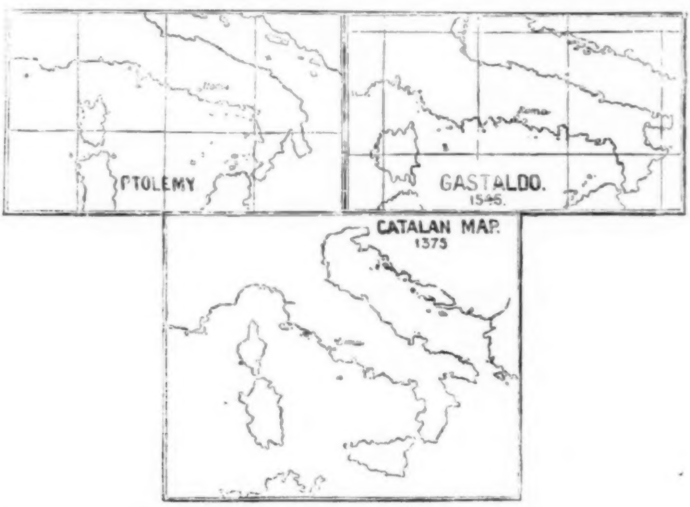
Another step in the right direction was taken when the cartographers and pilots of Portugal and Spain returned to the crude projection of Dicæarch, Eratosthenes, and Marinus, which enabled them to lay down places according to latitude and longitude upon their "plane charts."

Germany, debarred from taking a share in the great maritime discoveries of the age, indirectly contributed to their success by improvements in mathematical geography and the introduction of superior instruments. The navigators of the early middle ages still made use of an astrolabe when they desired to determine a latitude, but this instrument, which in the hands of an expert observer furnished excellent results on land, was of little use to a pilot stationed on the unsteady deck of a vessel. Regiomontanus consequently conferred an immense service upon the mariners of his time when, in 1471, he adapted to their use an instrument already known to the ordinary surveyors. It was this cross-staff which Martin Behaim introduced into the Portuguese navy, and which quickly made its way among the navigators of all countries. Most observations at sea were made with this simple instrument, variously modified in the course of ages, until it was superseded by Hadley's sextant. In the hands of the more skilful navigators of the seventeenth century, such as Baffin, James, and Tasman, the results obtained with the cross-staff were correct within two or three minutes.

Far greater difficulties were experienced in the observations of longitudes. Lunar eclipses were most generally made use of, but neither the ephemerides of Regiomontanus, for the years 1474 to 1506, which Columbus carried with him on his voyages, nor those of Peter Apianus, for 1521–70, were sufficiently accurate to admit of satisfactory results, even though the actual observation left nothing to be desired. Errors of 30° in longitude were by no means rare, and it was only when Kepler had published his "Rudolphine Tables" (1626), which according to Lalande formed the basis of all astronomical calculations during a century, that more exact results were obtained. The suggestion to determine longitude by means of lunar distances or occultations of stars bore no fruit at that time, as the knowledge of the complicated motion of the moon was still very imperfect. Still less was known about the movements of the satellites of Jupiter, which Galileo had first espied in 1610 when looking at that planet through his telescope. They became available only after tables of their revolutions and eclipses had been published by Cassini in 1668.

Another suggestion for the determination of longitude was made by Gemma Frisius in 1530—namely, that a clock or timekeeper should be employed for the purpose. One of Huygens's pendulum clocks was actually carried by Holmes to the Gulf of Guinea, but the results obtained were far from encouraging.

The difficulties which still attended the determination of longitude in the sixteenth century are conspicuously illustrated by the abortive attempts of a Congress of Spanish and Portuguese navigators who met at Badajoz and Yelves in 1524 for the purpose of laying down the boundary line, which Pope Alexander VI. had drawn at a distance of 370 Spanish leagues to the west of Cape Verde Islands, to separate the dominions of Spain from those of Portugal. Not being able to agree either as to the length of a degree, nor even as to that of a league, they separated without settling the question placed before them.



So uncertain were the results of observations for longitude made during the sixteenth and seventeenth centuries, that it was thought advisable to trust to the results of dead-reckoning rather than to those of celestial observations. But the method of dead-reckoning is available only when we have a knowledge of the size of the earth, and this knowledge was still very imperfect, notwithstanding the renewed measurement of an arc of the meridian by Snellius, the Dutch mathematician (1615). This measurement, however, is remarkable on account of its having for the first time applied the exact method of triangulation to a survey.

The problem of measuring the ship's way had been attempted by the Romans, who dragged paddle-wheels behind their ships, the revolutions of which enabled them to estimate the distance which the ship had travelled. But time, the strength of the wind, and the pilot's knowledge of the qualities of his ship, still constituted the principal elements for calculations of this kind, for the "catena a poppa" [which Magellan attached to the stern of his ship was merely intended to indicate the ship's leeway, and not the distance which it had travelled. The log, which for the first time enabled the mariner to carry out his dead-reckoning with confidence, is first described in Bourne's "Regiment for the Sea," which was published in 1577.

The eminent position which Italian cartographers occupied during the fourteenth and fifteenth centuries had to be surrendered by them, in the beginning of the sixteenth, to their pupils, the Portuguese and Spaniards, upon whom extensive voyages and discoveries had conferred exceptional advantages. These, in turn, had to yield to the Germans, and later on to the Dutch, who were specially qualified to become the reformers of cartography by their study of mathematics and of the ancient geographers, as also by the high degree of perfection which the arts of engraving on wood and copper had attained among them. German mathematicians first ventured to introduce the long-neglected geographical projections of Hipparchus and Ptolemy, and devised others of their own. Werner of Nürnberg (1514) invented an equivalent heart-shaped projection, whilst both Apianus and Staben (1520 and 1522) suggested equivalent projections. Still greater were the services of Gerhard Cremer, or Mercator (1512–94), the Ptolemy of the sixteenth century, who not only introduced the secant conical projection, but also invented that still known by his name, which was calculated to render such great service to the navigator, but was nevertheless not universally accepted until the middle of the fifteenth century, when the mediæval compass and plane charts finally disappeared.

The German cartographers of that age are to be commended, not because they copied Ptolemy's maps—for in this they had been preceded by others—but because they adopted his scientific methods in producing maps of their own. Their reforms began at home, as all reforms should. They were amply supported in their efforts by the many astronomers of note of whom Germany then boasted, and by quite a staff of local "geographers," of whom nearly every district of the empire boasted the possession of one. Among these local maps, that of Bavaria, by Philipp Bienewitz, or Apianus (1566), holds a distinguished rank, for it is the first map on a large scale (1 : 144,000) based upon a regular survey. Its errors in latitude do not exceed 1′, and those in longitude 3′, which is marvellously correct considering the age of its production. Like most maps of the period, it is engraved on wood, for though the art of engraving on copper was invented in Germany before 1446, and the first map was engraved there in 1450, copper engraving only became general at a much later date.

Perhaps the earliest general map of Germany, and certainly one of the most interesting, was that which the famous Cardinal Nicolas of Cues or Cusa completed in 1464, the only existing copy of which is to be found in the British Museum, where it was "discovered" by Baron Nordenskiöld. Mercator's map of Germany, published more than a century after that of the learned Cardinal (in 1585), was naturally far more complete in all respects, and was certainly far superior to the maps of any other country existing at that time. This fact is brought home to us by an inspection of a collection of maps to be found in the well-known "Theatrum Orbis" of Ortelius (first published in 1570), where we may see that the maps supplied by Humphrey Lloyd and other British cartographers are still without degree lines.

But when we follow Mercator, or, in fact, any other cartographer of the period, into regions the successful delineation of which depended upon an intelligent interpretation of itineraries and of other information collected by travellers, they are found to fail utterly. Nowhere is this utter absence of the critical faculty more glaringly exhibited than in the maps of Africa of that period.

Among the Dutch cartographers of that age one of the foremost places must be accorded to Waghenaer of Enkhuisen, whose "Mirror of the Sea," a collection of charts published in 1583, enjoyed a considerable reputation among British seamen. Other famous Dutch publishers of charts were Ortelius, Janssen, Blaeuw, and Vischer, who accumulated large stocks of copper plates, which constituted valuable heirlooms, and, not unlike the plates of certain modern map-publishers, supplied edition after edition without undergoing any change, except perhaps that of the date.

The age of great discoveries was past. All blanks upon our maps had not yet been filled up, but the contours of the great continents stood out distinctly, and in the main correctly. Discoveries on a large scale had become impossible, except in the Polar regions and in the interior of some of the continents; but greater preciseness had to be given to the work already done, and many details remained to be filled in. In this "age of measurements," as Peschel significantly calls it, better instruments, and methods of observation superior to those which had sufficed hitherto, were needed, and were readily forthcoming.

Picard, by making use of the telescope in measuring angles (1667), obtained results of a degree of accuracy formerly quite unattainable, even with instruments of huge proportions. For the theodolite, that most generally useful surveying instrument, we are indebted to Jonathan Sission (1737 or earlier). More important still, at all events to the mariner, was the invention of the sextant, generally ascribed to Hadley (1731), but in reality due to the genius of Newton. Equally important was the production of a trustworthy chronometer by John Harrison (1761), which first made possible the determination of meridian distances, and is invaluable whenever a correct knowledge of the time is required. One other instrument, quite recently added to the apparatus of the surveyor, is the photographic camera, converted for his especial benefit into a photogrammeter. This instrument has not as yet been utilized for ascertaining the relative positions of celestial bodies, but has already done excellent service in ordinary surveying, especially when it is required to portray the sides of inaccessible mountains.

But the full fruits of these inventions could be enjoyed only after Bradley had discovered the aberration of light (1728) and the nutation of the earth's axis (1747); Domenique Cassini had furnished trustworthy tables of the refraction of light; and the complicated movement of the moon had been computed by Euler (1746), Tobias Mayer (1753), Bradley (1770), and, more recently, by Hansen.

Positively novel methods for determining the latitude and longitude of a place can scarcely be said to have been proposed during this period, but many of the older methods only became really available after the improvements in the instruments indicated above had taken place, and the computations had been freed from the errors which vitiated them formerly.

Real progress, however, has been made in the determination of altitudes. Formerly they could be ascertained only by trigonometrical measurement, or by a laborious process of levelling, but since physicists have shown how the decrease of atmospheric pressure with the altitude, and the boiling-point of water depending upon this decrease, afforded a ready means of determining heights, the barometer, aneroid, and boiling-point thermometer have become the indispensable companions of the explorer, and our knowledge of the relief of the land has advanced rapidly.

Equally rapid have been the improvements in our instruments for measuring the depth of the ocean, since a knowledge of the configuration of its bed was demanded by the practical requirements of the telegraph engineers.

And in proportion as the labours of the surveyors and explorers gained in preciseness, so did the cartographer of the age succeed in presenting the results achieved in a manner far more satisfactory than had been done by his predecessors. His task was comparatively easy so long as he only dealt with horizontal dimensions, though even in the representation of these a certain amount of skill and judgment are required to make each feature tell in proportion to its relative importance. The delineation of the inequalities of the earth's surface, however, presented far greater difficulties. The mole-hills or serrated ridges, which had not yet quite disappeared from our maps in the beginning of this century, failed altogether in doing justice to our actual knowledge. The first timid attempt to represent hills as seen from a bird's-eye view, and of shading them according to the steepness of their slopes, appear on a map of the Breisgau, published by Homann in 1718. We find this system fully developed on La Condamine's map of Quito, published in 1751, and it was subsequently popularized by Arrowsmith. In this crude system of hill shading, however, everything was left to the judgment of the draughtsman, and only after Lehmann (1783) had superimposed it upon a groundwork of contours, and had regulated the strength of the hatching in accordance with the degree of declivity to be represented did it become capable of conveying a correct idea of the configuration of the ground.

The first to fully recognize the great importance of contours was Philip Buache, who had prepared a contoured map of the Channel in 1737, and suggested that the same system might profitably be extended to a delineation of the relief of the land; and this idea, subsequently taken up by Ducarla of Vabres, was for the first time carried into practice by Dupain-Triel, who published a contoured map of France in 1791. Up to the present time more than eighty methods of showing the hills have been advocated, but it may safely be asserted that none of these

methods can be mathematically correct unless it is based upon horizontal contours.

The credit of having done most towards the promotion of cartography in the course of the eighteenth century belongs to France. It was France which first equipped expeditions to determine the size of the earth; France which produced the first topographical map based upon scientific survey—a work begun by César François Cassini in 1744, and completed by his son five years after his father's death; it was France, again, which gave birth to D'Anville, the first critical cartographer whom the world had ever seen.

Delisle (1675–1726), a pupil of Cassini's, had already been able to rectify the maps of the period by utilizing the many astronomical observations which French travellers had brought home from all parts of the world. This work of reform was carried further by D'Anville (1697–1782), who swept away the fanciful lakes from off the face of Africa, thus forcibly bringing home to us the poverty of our knowledge; who boldly refused to believe in the existence of an Antarctic continent covering half the southern hemisphere, and always brought sound judgment to bear upon the materials which the ever-increasing number of travellers placed at his disposal. And whilst France led the way, England did not lag far behind.

In that country the discoveries of Cook and of other famous navigators, and the spread of British power in India, gave the first impulse to a more diligent cultivation of the art of representing the surface of the earth on maps. There, to a greater extent than on the Continent, the necessities of the navigator called into existence a vast number of charts, amongst which are many hundreds of sheets published by Dalrymple and Joseph Desbarres (1776). Faden, one of the most prolific publishers of maps, won distinction, especially for his county maps, several of which, like that of Surrey by Linley and Gardner, are based upon trigonometrical surveys carried on by private individuals. England was the first to follow the lead of France in undertaking a regular topographical survey (1785). Nor did she lack critical cartographers. James Rennell (born 1742) sagaciously arranged the vast mass of important information collected by British travellers in India and Africa; but it is chiefly the name of Aaron Arrowsmith (died 1823) with which the glory of the older school of English cartographers is most intimately connected. Arrowsmith became the founder of a family of geographers, whose representative in the third generation, up to the date of his death in 1873, worthily upheld the ancient reputation of the family. Another name which deserves to be gratefully remembered is that of John Walker, to whom the charts published by our Admiralty are indebted for that perspicuous, firm, and yet artistic execution which, whilst it enhances their scientific value, also facilitates their use by the mariner.

Since the beginning of the present century Germany has once more become the head-quarters of scientific cartography; and this is due as much to the inspiriting teachings of a Ritter and a Humboldt as to the general culture and scientific training, combined with technical skill, commanded by the men who more especially devoted themselves to this branch of geography, which elsewhere was too frequently allowed to fall into the hands of mere mechanics. Men like Berghaus, Henry Kiepert, and Petermann, the best known pupil of the first of these, must always occupy a foremost place in the history of our department of knowledge. Berghaus, who may be truly described as the founder of the modern school of cartography, and who worked under the immediate inspiration of a Ritter and a Humboldt, presented us with the first comprehensive collection of physical maps (1837). Single maps of this kind had, no doubt, been published before—Kircher (1665) had produced a map of the ocean currents, Edmund Halley (1686) had embodied the results of his own researches in maps of the winds and of the variation of the compass (1686), whilst Ritter himself had compiled a set of physical maps (1866)—but no work of the magnitude of Berghaus's famous "Physical Atlas" had seen the light before. Nor could it have been published even then had it not been for the unstinted support of a firm like that of Justus Perthes, already the publisher of Stieler's "Atlas" (1817–23), and subsequently of many other works which have carried its fame into every quarter of the globe.

And now, at the close of this nineteenth century, we may fairly boast that the combined science and skill of surveyors and cartographers, aided as they are by the great advance of the graphic arts, are fully equal to the production of a map which shall be a faithful image of the earth's surface. Let us imagine for one moment that an ideal map of this kind were before us, a map exhibiting not merely the features of the land and the depth of the sea, but also the extent of forests and of pasture-lands, the distribution of human habitations, and all those features the representation of which has become familiar to us through physical and statistical atlases. Let us then analyze the vast mass of facts thus placed before us, and we shall find that they form quite naturally two well-defined divisions—namely, those of physical and political geography—whilst the third department of our science, mathematical geography, deals with the measurement and survey of our earth, the ultimate outcome of which is the production of a perfect map.

I shall abstain from giving a laboured definition of what I consider geography should embrace, for definitions of this kind help practical workers but little, and will never deter anyone who feels disposed and capable from straying into fields which an abuse of logic has clearly demonstrated to lie outside his proper domain. But I wish to enforce the fact that topography and chorography, the description of particular places or of entire countries, should always be looked upon as integral portions of geographical research. It is they which furnish many of the blocks needed to rear our geographical edifice, and which constitute the best training school for the education of practical geographers, as distinguished from mere theorists.

That our maps, however elaborate, should be supplemented by descriptions will not even be gainsaid by those who are most reluctant to grant us our independent existence among the sciences which deal with the earth and man who inhabits it. This concession, however, can never content us. We cannot allow ourselves to be reduced to the position of collectors of facts. We claim the right to discuss ourselves the facts we have collected, to analyze them, to generalize from them, and to trace the correlations between cause and effect. It is thus that geography becomes comparative; and whilst comparative physical geography, or morphology, seeks to explain the origin of the existing surface features of our earth, comparative political geography, or anthropo-geography, as it is called by Dr. Ratzel, one of the most gifted representatives of geographical science in Germany, deals with man in relation to the geographical conditions which influence him. It is this department of geography which was so fruitfully cultivated by Karl Ritter.

Man is indeed in a large measure "the creature of his environment," for who can doubt for a moment that geographical conditions have largely influenced the destinies of nations, have directed the builders of our towns, determined the paths of migrations and the march of armies, and have impressed their stamp even upon the character of those who have been subjected to them for a sufficiently extended period? . . . .

It must not, however, be assumed for one moment that the dependence of man upon Nature is absolute. The natural resources of a country require for their full development a people of energy and capacity; and instances in which they have been allowed to lie dormant, or have been wasted, are numerous. . . .

Perhaps one of the most instructive illustrations of the complex human agencies which tend to modify the relative importance of geographical conditions is presented to us by the Mediterranean. The time when this inland sea was the centre of civilization and of the world's commerce, whilst the shores of Western Europe were only occasionally visited by venturesome navigators or conquering Roman hosts, does not lie so very far behind us. England, at that period, turned her face towards Continental Europe, of which it was a mere dependency. The prosperity of the Mediterranean countries survived far into the middle ages, and Italy at one time enjoyed the enviable position of being the great distributor of the products of the East, which found their way across the Alps into Germany, and through the gates of Gibraltar to the exterior ocean. But a change was brought about, partly through the closing of the old Oriental trade routes, consequent upon the conquests of the Turks, partly through the discovery of a new world and of a maritime highway to India. When Columbus, himself an Italian, returned from the West Indies in 1493, and Vasco da Gama brought the first cargo of spices from India in 1499, the star of Italy began to fade. And whilst the spices of the Indies and the gold of Guinea poured wealth into the lap of Portugal, and Spain grew opulent on the silver mines of Mexico and Peru, Venice was

vainly beseeching the Sultan to reopen the old trade route through the Red Sea. The dominion of the sea had passed from Italy to Spain and Portugal, and passed later on to the Dutch and English. But mark how the great geographical discoveries of that age affected the relative geographical position of England! England no longer lay on the skirts of the habitable world, it had become its very centre. And this natural advantage was enhanced by the colonial policies of Spain and Portugal, who exhausted their strength in a task far beyond their powers, took possession of tropical countries only, and abandoned to England the less attractive but in reality far more valuable regions of North America. England was thus enabled to become the founder of real colonies, the mother of nations: and her language, customs, and political institutions found a home in a new world.

And now, when the old highway through the Red Sea has been reopened, when the wealth flowing through the Canal of Suez is beginning to revivify the commerce of Italy, England may comfort herself with the thought that in her own colonies and in the States which have sprung up across the Atlantic she may find ample compensation for any possible loss that may accrue to her through geographical advantages being once more allowed to have full play.

I am afraid I have unduly tried your patience. I believe you will agree with me that no single individual can be expected to master all those departments which are embraced within the wide field of geography. Even the master-mind of a Humboldt fell short of this, and facts have accumulated since his time at an appalling rate. All that can be expected of our modern geographer is that he should command a comprehensive general view of his field, and that he should devote his energies and capacities to the thorough cultivation of one or more departments that lie within it.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY PROF. F. MAX MÜLLER, PRESIDENT OF THE SECTION.

IT was forty-four years ago that for the first and for the last time I was able to take an active part in the meetings of the British Association for the Advancement of Science. It was at Oxford, in 1847, when I read a paper on the "Relation of Bengali to the Aryan and Aboriginal Languages of India," which received the honour of being published in full in the Transactions of the Association for that year. I have often regretted that absence from England and pressure of work have prevented me year after year from participating in the meetings of the Association. But, being a citizen of two countries—of Germany by birth, of England by adoption—my long vacations have generally drawn me away to the Continent, so that to my great regret I found myself precluded from sharing either in your labours or in your delightful social gatherings.

I wonder whether any of those who were present at that brilliant meeting at Oxford in 1847 are present here to-day. I almost doubt it. Our President then was Sir Robert Inglis, who will always be known in the annals of English history as having been preferred to Sir Robert Peel as Member of Parliament for the University of Oxford. Among other celebrities of the day I remember Sir Roderick Murchison, Sir David Brewster, Dean Buckland, Sir Charles Lyell, Prof. Sedgwick, Prof. Owen, and many more—a galaxy of stars, all set or setting. Young Mr. Ruskin acted as Secretary to the Geological Section. Our Section was then not even recognized as yet as a Section. We ranked as a sub-Section only of Section D, *Zoology* and *Botany*. We remained in that subordinate position till 1851, when we became Section E, under the name of *Geography* and *Ethnology*. From 1869, however, Ethnology seems almost to have disappeared again, being absorbed in Geography, and it was not till the year 1884 that we emerged once more as what we are to-day, Section H, or *Anthropology*.

In the year 1847 our sub-Section was presided over by Prof. Wilson, the famous Sanskrit scholar. The most active debaters, so far as I remember, were Dr. Prichard, Dr. Latham, and Mr. Crawfurd, well known then under the name of the Objector-General. I was invited to join the meeting by Bunsen, then Prussian Minister in London, who also brought with him his friend Dr. Karl Meyer, the Celtic scholar. Prince Albert was present at our debates, so was Prince Louis Lucien Bonaparte. Our Ethnological sub-Section was then most popular, and attracted very large audiences.

When looking once more through the debates carried on in our Section in 1847, I was very much surprised when I saw how very like the questions which occupy us to-day are to those which we discussed in 1847. I do not mean to say that there has been no advance in our science. Far from it. The advance of linguistic, ethnological, anthropological, and biological studies, all of which claim a hearing in our Section, has been most rapid. Still that advance has been steady and sustained; there has been no cataclysm, no deluge, no break in the advancement of our science, and nothing seems to me to prove its healthy growth more clearly than this uninterrupted continuity, which unites the past with the present, and will, I hope, unite the present with the future.

No paper is in that respect more interesting to read than the address which Bunsen prepared for the meeting in 1847, and which you will find in the Transactions of that year. Its title is "On the Results of the recent Egyptian Researches in reference to Asiatic and African Ethnology, and the Classification of Languages." But you will find in it a great deal more than what this title would lead you to expect.

There are passages in it which are truly prophetic, and which show that, if prophecy is possible anywhere, it is possible, nay, it ought to be possible, in the temple of Science, and under the inspiring influence of knowledge and love of truth.

Allow me to dwell for a little while on this remarkable paper. It is true, we have travelled so fast that Bunsen seems almost to belong to ancient history. This very year is the hundredth anniversary of his birth, and this very day the centenary of his birth is being celebrated in several towns of Germany. In England also his memory should not be forgotten. No one, not being an Englishman by birth, could, I believe, have loved this country more warmly, and could have worked more heartily than Bunsen did to bring about that friendship between England and Germany which must for ever remain the corner-stone of the peace of Europe, and the *sine quâ non* of that advancement of science to which our Association is devoted. His house in Carlton Terrace was a true international academy, open to all who had something to say, something worth listening to, a kind of sanctuary against vulgarity in high places, a neutral ground where the best representatives of all countries were welcome and felt at home. But this also belongs to ancient history. And yet, when we read Bunsen's paper, delivered in 1847, it does not read like ancient history. It deals with the problems which are still in the foreground, and if it could be delivered again to-day by that genial representative of German learning, it would rouse the same interest, provoke the same applause, and possibly the same opposition also, which it roused nearly half a century ago. Let me give you a few instances of what I mean.

We must remember that Darwin's "Origin of Species" was published in 1859, his "Descent of Man" in 1871. But here in the year 1847 one of the burning questions which Bunsen discusses is the question of the possible descent of man from some unknown animal. He traces the history of that question back to Frederick the Great, and quotes his memorable answer to D'Alembert. Frederick the Great, you know, was not disturbed by any qualms of orthodoxy. "In my kingdom," he used to say, "everybody may save his soul according to his own fashion." But when D'Alembert wished him to make what he called the *salto mortale* from monkey to man, Frederick the Great protested. He saw what many have seen since, that there is no possible transition from reasonlessness to reason, and that with all the likeness of their bodily organs there is a barrier which no animal can clear, or which, at all events, no animal has as yet cleared. And what does Bunsen himself consider the real barrier between man and beast? "It is language," he says, "which is unattainable, or, at least, unattained, by any animal except man." In answer to the argument that, given only a sufficient number of years, a transition by imperceptible degrees from animal cries to articulate language is at least conceivable, he says:—"Those who hold that opinion have never been able to show the possibility of the first step. They attempt to veil their inability by the easy but fruitless assumption of an infinite space of time, destined to explain the gradual development of animals into men; as if millions of years could supply the want of the agent necessary for the first movement, for the first step, in the line of progress! No numbers can effect a logical impossibility.

How, indeed, could reason spring out of a state which is destitute of reason? How can speech, the expression of thought, develop itself, in a year, or in millions of years, out of inarticulate sounds, which express feelings of pleasure, pain, and appetite?"

He then appeals to Wilhelm von Humboldt, whom he truly calls the greatest and most acute anatomist of almost all human speech. Humboldt goes so far as to say:—"Rather than assign to all language a uniform and mechanical march that would lead them step by step from the grossest beginnings to their highest perfection, I should embrace the opinion of those who ascribe the origin of language to an immediate revelation of the Deity. They recognize at least that divine spark which shines through all idioms, even the most imperfect and the least cultivated."

Bunsen then sums up by saying: "To reproduce Monboddo's theory in our days, after Kant and his followers, is a sorry anachronism, and I therefore regret that so low a view should have been taken of the subject lately in an English work of much correct and comprehensive reflection and research respecting natural science." This remark refers, of course, to the "Vestiges of Creation" (see an article in the *Edinburgh Review*, July, 1845), which was then producing the same commotion which Darwin's "Origin of Species" produced in 1859.

Bunsen was by no means unaware that in the vocal expression of feelings, whether of joy or pain, and in the imitation of external sounds, animals are on a level with man. "I believe with Kant," he says, "that the formation of ideas or notions, embodied in words, presupposes the action of the senses and impressions made by outward objects on the mind. But," he adds, "what enables us to see the genus in the individual, the whole in the many, and to form a word by connecting a subject with a predicate, is the power of the mind, and of this the brute creation exhibits no trace."

You know how for a time, and chiefly owing to Darwin's predominating influence, every conceivable effort was made to reduce the distance which language places between man and beast, and to treat language as a vanishing line in the mental evolution of animal and man. It required some courage at times to stand up against the authority of Darwin, but at present all serious thinkers agree, I believe, with Bunsen, that no animal has developed what we mean by rational language, as distinct from mere utterances of pleasure or pain, from imitation of sounds and from communication by means of various signs, a subject that has lately been treated with great fullness by my learned friend Prof. Romanes in his "Mental Evolution of Man." Still, if all true science is based on facts, the fact remains that no animal has ever formed what we mean by a language; and we are fully justified, therefore, in holding with Bunsen and Humboldt, as against Darwin and Prof. Romanes, that there *is* a specific difference between the human animal and all other animals, and that that difference consists in language as the outward manifestation of what the Greeks meant by *Logos*.

Another question which occupies the attention of our leading anthropologists is the proper use to be made of the languages, customs, laws, and religious ideas of so-called savages. Some, as you know, look upon these modern savages as representing human nature in its most primitive state, while others treat them as representing the lowest degeneracy into which human nature may sink. Here, too, we have learnt to distinguish. We know that certain races have had a very slow development, and may, therefore, have preserved some traces of those simple institutions which are supposed to be characteristic of primitive life. But we also know that other races have degenerated and are degenerating even now. If we hold that the human race forms but one species, we cannnot, of course, admit that the ancestors even of the most savage tribes, say of the Australians, came into the world one day later than the ancestors of the Greeks, or that they passed through fewer evolutions than their more favoured brethren. The whole of humanity would be of exactly the same age. But we know its history from a time only when it had probably passed already through many ups and downs. To suppose, therefore, that the modern savage is the nearest approach to primitive man would be against all the rules of reasoning. Because in some countries, and under stress of unfavourable influences, some human tribes have learnt to feed on human flesh, it does not follow that our first ancestors were cannibals. And here, too, Bunsen's words have become so strikingly true that I may be allowed to quote them: "The savage is justly disclaimed as the prototype of natural, original man; for linguistic inquiry shows that the languages of savages are degraded and decaying fragments of nobler formations."

I know well that in unreservedly adopting Bunsen's opinion on this point also I run counter to the teaching of such well-known writers as Sir John Lubbock, Reclus, and others. It might be supposed that Mr. Herbert Spencer also looked upon savages as representing the primitive state of mankind. But if he ever did so, he certainly does so no longer, and there is nothing I admire so much in Mr. Herbert Spencer as this simple love of truth, which makes him confess openly whenever he has seen occasion to change his views. "What terms and what conceptions are truly primitive," he writes, "would be easy if we had an account of truly primitive men. But there are sundry reasons for suspecting that existing men of the lowest type forming social groups of the simplest kind do not exemplify men as they originally were. Probably most of them, if not all, had ancestors in a higher state" (*Open Court*, No. 205, p. 2896).

Most important also is a hint which Bunsen gives that the students of language should follow the same method which has been followed with so much success in geology; that they should begin with studying the modern strata of speech, and then apply the principles, discovered there, to the lower or less accessible strata. It is true that the same suggestion had been made by Leibniz, but many suggestions are made and are forgotten again, and the merit of rediscovering an old truth is often as great as the discovery of a new truth. This is what Bunsen said: "In order to arrive at the law which we are endeavouring to find (the law of the development of language) let us first assume, as geology does, that the same principles which we see working in the (recent) development were also at work at the very beginning, modified in degree and in form, but essentially the same in kind." We know how fruitful this suggestion has proved, and how much light an accurate study of modern languages and of spoken dialects has thrown on some of the darkest problems of the science of language. But fifty years ago it was Sanskrit only, or Hebrew, or Chinese, that seemed to deserve the attention of the students of comparative philology. Still more important is Bunsen's next remark, that language begins with the sentence, and that in the beginning each word was a sentence in itself. This view also has found strong supporters at a later time—for instance, my friend Prof. Sayce—though at the time we are speaking of it was hardly thought of. I must here once more quote Bunsen's own words: "The supreme law of progress in all language shows itself to be the progress from the substantial isolated word, as an undeveloped expression of a whole sentence, towards such a construction of language as makes every single word subservient to the general idea of a sentence, and shapes, modifies, and dissolves it accordingly."

And again: "Every sound in language must originally have been significative of something. The unity of sound (the syllable, pure or consonantised) must therefore originally have corresponded to a unity of conscious plastic thought, and every thought must have had a real or substantial object of perception. . . . Every single word implies necessarily a complete proposition, consisting of subject, predicate, and copula."

This is a most pregnant remark. It shows as clearly as daylight the enormous difference there is between the mere utterance of the sound *Pah* and *Mah*, as a cry of pleasure or distress, and the pronunciation of the same syllable as a sentence, when *Pah* and *Mah* are meant for "This is *Pah*," "This is *Mah*"; or, after a still more characteristic advance of the human intellect, "This is *a Pah*," "This is *a Mah*," which is not very far from saying, "This man belongs to the class or genus of fathers."

Equally important is Bunsen's categorical statement that everything in language must have been originally significant, that everything formal must originally have been substantial. You know what a bone of contention this has been of late between what is called the old school and the new school of comparative philology. The old school maintained that every word consisted of a root and of certain derivative suffixes, prefixes, and infixes. The modern school maintained that there existed neither roots by themselves nor suffixes, prefixes, and infixes by themselves, and that the theory of agglutination—of gluing suffixes to roots—was absurd. The old school looked upon these suffixes as originally independent and significative words; the modern school declined to accept this view except in a few irrefragable instances. I think the more accurate reasoners are coming back to the opinion held by the old school, that all formal elements of language were originally substantial,

and therefore significative; that they are the remnants of predicative or demonstrative words. It is true we cannot always prove this as clearly as in the case of such words as *hard-ship, wis-dom, man-hood*, where *hood* can be traced back to *hád*, which in Anglo-Saxon exists as an independent word, meaning state or quality. Nor do we often find that a suffix like *mente*, in *claramente, clairement*, continues to exist by itself, as when we say in Spanish *clara, concisa y elegantemente*. It is perfectly true that the French, when they say that a hammer falls *lourdement*, or heavily, do not deliberately take the suffix *ment*—originally the Latin *mente*, "with a mind"—and glue it to their adjective *lourd*. Here the new school has done good service in showing the working of that instinct of analogy which is a most important element in the historical development of human speech. One compound was formed in which *mente* retained its own meaning; for instance, *forti mente*, "with a brave mind." But when this had come to mean *bravely*, and no more, the working of analogy began; and if *fortement*, from *fort*, could mean "bravely," then why not *lourdement*, from *lourd*, "heavily?" But in the end there is no escape from Bunsen's fundamental principle that everything in language was originally language—that is, was significative, was substantial, was material—before it became purely formal.

But it is not only with regard to these general problems that Bunsen has anticipated the verdict of our own time. Some of his answers to more special questions also show that he was right when many of his contemporaries, and even successors, were wrong. It has long been a question, for instance, whether the Armenian language belonged to the Iranic branch of the Aryan family, or whether it formed an independent branch, like Sanskrit, Persian, or Greek. Bunsen, in 1847, treated Armenian as a separate branch of Aryan speech; and that it is so was proved by Prof. Hübschmann in 1883.

Again, there has been a long controversy whether the language of the Afghans belonged to the Indic or the Iranic branch. Dr. Trumpp tried to show that it belonged, by certain peculiarities, to the Indic or Sanskritic branch. Prof. Darmesteter has proved but lately that it shares its most essential characteristics in common with Persian. Here, too, Bunsen guessed rightly—for I do not mean to say that it was more than a guess—when he stated that "Pushtu, the language of the Afghans, belongs to the Persian branch."

I hope you will forgive me for having detained you so long with a mere retrospect. I could not deny myself the satisfaction of paying this tribute of gratitude and respect to my departed friend, Baron Bunsen. To have known him belongs to the most cherished recollections of my life. But though I am myself an old man—much older than Bunsen was at our meeting in 1847—do not suppose that I came here as a mere *laudator temporis acti*. Certainly not. If one tries to recall what anthropology was in 1847, and then considers what it is now, its progress seems most marvellous. I do not think so much of the new materials which have been collected from all parts of the world. These last fifty years have been an age of discovery in Africa, in Central Asia, in America, in Polynesia, and in Australia, such as can hardly be matched in any previous century.

But what seems to me even more important than the mere increase of material is the new spirit in which anthropology has been studied during the last generation. I do not mean to depreciate the labours of so-called *dilettanti*. After all, *dilettanti* are lovers of knowledge, and in a study such as the study of anthropology the labours of these volunteers, or *franc-tireurs*, have often proved most valuable. But the study of man in every part of the world has ceased to be a subject for curiosity only. It has been raised to the dignity, but also to the responsibility, of a real science, and it is now guided by principles as strict and as rigorous as any other science—such as zoology, botany, mineralogy, and all the rest. Many theories which were very popular fifty years ago are now completely exploded; nay, some of the very principles by which our science was then guided have been discarded. Let me give you one instance—perhaps the most important one—as determining the right direction of anthropological studies.

At our meeting in 1847 it was taken for granted that the study of comparative philology would be in future the only safe foundation for the study of anthropology. Linguistic ethnology was a very favourite term used by Bunsen, Prichard, Latham, and others. It was, in fact, the chief purpose of Bunsen's paper to show that the whole of mankind could be classified according to language. I protested against this view at the time, and in 1853 I published my formal protest in a letter to Bunsen, "On the Turanian Languages." In a chapter called "Ethnology *versus* Phonology" I called, if not for a complete divorce, at least for a judicial separation between the study of philology and the study of ethnology. "Ethnological race," I said, "and phonological race are not commensurate, except in ante-historical times, or, perhaps, at the very dawn of history. With the migration of tribes, their wars, their colonies, their conquests and alliances, which, if we may judge from their effects, must have been much more violent in the ethnic than ever in the political periods of history, it is impossible to imagine that race and language should continue to run parallel. The physiologist should pursue his own science, unconcerned about language. Let him see how far the skulls, or the hair, or the colour, or the skin of different tribes admit of classification; but to the sound of their words his ear should be as deaf as that of the ornithologist's to the notes of caged birds. If his Caucasian class includes nations or individuals speaking Aryan (Greek), Turanian (Turkish), and Semitic (Hebrew) languages, it is not his fault. His system must not be altered to suit another system. There is a better solution both for his difficulties and for those of the phonologist than mutual compromise. The phonologist should collect his evidence, arrange his classes, divide and combine as if no Blumenbach had ever looked at skulls, as if no Camper had ever measured facial angles, as if no Owen had ever examined the basis of a cranium. His evidence is the evidence of language, and nothing else; this he must follow, even though in the teeth of history, physical or political. . . . There ought to be no compromise between ethnological and phonological science. It is only by stating the glaring contradictions between the two that truth can be elicited."

At first my protest met with no response; nay, curiously enough, I have often been supposed to be the strongest advocate of the theory which I so fiercely attacked. Perhaps I was not entirely without blame, for, having once delivered my soul, I allowed myself occasionally the freedom to speak of the Aryan or the Semitic race, meaning thereby no more than the people, whoever and whatever they were, who spoke Aryan or Semitic languages. I wish we could distinguish in English as in Hebrew between *nations* and *languages*. Thus in the Book of Daniel, iii. 4, "the herald cried aloud, . . . O people, nations, and languages." Why then should we not distinguish between nations and languages? But to put an end to every possible misunderstanding, I declared at last that to speak of "an Aryan skull would be as great a monstrosity as to speak of a dolichocephalic language."

I do not mean to say that this old heresy, which went by the name of linguistic ethnology, is at present entirely extinct. But among all serious students, whether physiologists or philologists, it is by this time recognized that the divorce between ethnology and philology, granted if only for incompatibility of temper, has been productive of nothing but good.

Instead of attempting to classify mankind as a whole, students are now engaged in classing skulls, in classing hair, and teeth, and skin. Many solid results have been secured by these special researches; but, as yet, no two classifications, based on these characteristics, have been made to run parallel.

The most natural classification is, no doubt, that according to the colour of the skin. This gives us a black, a brown, a yellow, a red, and a white race, with several subdivisions. This classification has often been despised as unscientific; but it may still turn out far more valuable than is at present supposed.

The next classification is that by the colour of the eyes, as black, brown, hazel, grey, and blue. This subject also has attracted much attention of late, and, within certain limits, the results have proved very valuable.

The most favourite classification, however, has always been that according to the skulls. The skull, as the shell of the brain, has by many students been supposed to betray something of the spiritual essence of man; and who can doubt that the general features of the skull, if taken in large averages, do correspond to the general features of human character? We have only to look round to see men with heads like a cannon-ball and others with heads like a hawk. This distinction has formed the foundation for a more scientific classification into *brachycephalic*, *dolichocephalic*, and *mesocephalic* skulls. The proportion of 80 : 100 between the transverse and longitudinal diameter gives us the ordinary or mesocephalic type, the pro-

portion of 75 : 100 the dolichocephalic, the proportion of 85 : 100 the brachycephalic type. The extremes are 70 : 100 and 90 : 100.

If we examine any large collection of skulls, we have not much difficulty in arranging them under these three classes; but if, after we have done this, we look at the nationality of each skull, we find the most hopeless confusion. Pruner Bey, as Peschel tells us in his "Völkerkunde," has observed brachycephalic and dolichocephalic skulls in children born of the same mother; and if we consider how many women have been carried away into captivity by Mongolians in their inroads into China, India, and Germany, we cannot feel surprised if we find some longheads among the roundheads of those Central Asiatic hordes.

Only we must not adopt the easy expedient of certain anthropologists who, when they find dolichocephalic and brachycephalic skulls in the same tomb, at once jump to the conclusion that they must have belonged to two different races. When, for instance, two dolichocephalic and three brachycephalic skulls were discovered in the same tomb at Alexanderpol, we were told at once that this proved nothing as to the simultaneous occurrence of different skulls in the same family; nay, that it proved the very contrary of what it might seem to prove. It was clear, we were assured, that the two dolichocephalic skulls belonged to Aryan chiefs and the three brachycephalic skulls to their non-Aryan slaves, who were killed and buried with their masters, according to a custom well known to Herodotus. This sounds very learned, but is it really quite straightforward?

Besides the general division of skulls into dolichocephalic, brachycephalic, and mesocephalic, other divisions have been undertaken, according to the height of the skull, and, again, according to the maxillary and the facial angles. This latter division gives us *orthognathic*, *prognathic*, and *mesognathic* skulls.

Lastly, according to the peculiar character of the hair, we may distinguish two great divisions, the people with woolly hair (*Ulotriches*) and people with smooth hair (*Lissotriches*). The former are subdivided into *Lophocomi*, people with tufts of hair, and *Eriocomi*, or people with fleecy hair. The latter are divided into *Euthycomi*, straight-haired, and *Euplocami* (not *Euplocomic*, wavy-haired, as Brinton gives it), wavy-haired. It has been shown that these peculiarities of the hair depend on the peculiar form of the hair-tubes, which, in cross-sections, are found to be either round or elongated in different ways.

Now all these classifications, to which several more might be added, those according to the orbits of the eyes, the outlines of the nose, the width of the pelvis, are by themselves extremely useful. But few of them only, if any, run strictly parallel. It has been said that all dolichocephalic races are prognathic, and have woolly hair. I doubt whether this is true without exception; but, even if it were, it would not allow us to draw any genealogical conclusions from it, because there are certainly many dolichocephalic people who are not woolly-haired, as, for instance, the Eskimos (Brinton's "Races of People," p. 249).

Now, let us consider whether there can be any organic connection between the shape of the skull, the facial angle, the conformation of the hair, or the colour of the skin on one side, and what we call the great families of language on the other. That we speak at all may rightly be called a work of nature, *opera naturale*, as Dante said long ago; but that we speak thus or thus, *così o così*, that, as the same Dante said, depends on our pleasure—that is our work. To imagine, therefore, that as a matter of necessity, or as a matter of fact, dolichocephalic skulls have anything to do with Aryan, mesocephalic with Semitic, or brachycephalic with Turanian speech, is nothing but the wildest random thought; it can convey no rational meaning whatever. We might as well say that all painters are dolichocephalic, and all musicians brachycephalic, or that all lophocomic tribes work in gold, and all lissocomic tribes in silver.

If anything must be ascribed to prehistoric times, surely the differentiation of the human skull, the human hair, and the human skin, would have to be ascribed to that distant period. No one, I believe, has ever maintained that a mesocephalic skull was split or differentiated into a dolichocephalic and a brachycephalic variety in the bright sunshine of history.

But let us, for the sake of argument, assume that in prehistoric times all dolichocephalic people spoke Aryan, all mesocephalic, Semitic, all brachycephalic, Turanian languages: how would that help us?

So long as we know anything of the ancient Aryan, Semitic, and Turanian languages, we find foreign words in each of them. This proves a very close and historical contact between them. For instance, in Babylonian texts of 3000 B.C. there is the word *sindhu* for cloth made of vegetable fibres, linen. That can only be the Sk. *sindhu*, the Indus, or *saindhava* what comes from the Indus. It would be the same word as the Homeric σινδών, fine cloth ("Physical Religion," p. 87). In Egyptian we find so many Semitic words that it is difficult to say whether they were borrowed or derived from a common source. I confess I am not convinced, but Egyptologists of high authority assure us that the names of several Aryan peoples, such as the Sicilians and Sardinians, occur in the fourteenth century B.C., in the inscriptions of the time of Menephthah I. Again, as soon as we know anything of the Turanian languages—Finnish, for instance—we find them full of Aryan words. All this, it may be said, applies to a very recent period in the ancient history of humanity. Still, we have no access to earlier documents, and we may fairly say that this close contact which existed then existed, probably, at an earlier time also.

If, then, we have no reason to doubt that the ancestors of the people speaking Aryan, Semitic, and Turanian languages, lived in close proximity, would there not have been marriages between them so long as they lived in peace, and would they not have killed the men and carried off the women in time of war? What, then, would have been the effect of a marriage between a dolichocephalic mother and a brachycephalic father? The materials for studying this question of *métissage*, as the French call it, are too scanty as yet to enable us to speak with confidence. But whether the paternal or the maternal type prevailed, or whether their union gave rise to a new permanent variety, still it stands to reason that the children of a dolichocephalic captive woman might be found, after fifty or sixty years, speaking the language of the brachycephalic conquerors.

It has been the custom to speak of the early Aryan, Semitic, and Turanian races as large swarms—as millions pouring from one country into another. It has been calculated that these early nomads would have required immense tracts of meadow land to keep their flocks, and that it was the search for new pastures that drove them, by an irresistible force, over the whole inhabitable earth.

This may have been so, but it may also have not been so. Anyhow, we have a right to suppose that, before there were millions of human beings, there were at first a few only. We have been told of late that there never was a first man; but we may be allowed to suppose, at all events, that there were at one time a few first men and a few first women. If, then, the mixture of blood by marriage and the mixture of language in peace or war took place at that early time, when the world was peopled by some individuals, or by some hundreds, or by some thousands only, think what the necessary result would have been. It has been calculated that it would only require 600 years to populate the whole earth with the descendants of one couple, the first father being dolichocephalic and the first mother brachycephalic. They might, after a time, all choose to speak an Aryan language, but they could not choose their skulls, but would have to accept them from nature, whether dolichocephalic or brachycephalic.

Who, then, would dare at present to lift up a skull and say this skull must have spoken an Aryan language, or lift up a language and say this language must have been spoken by a dolichocephalic skull? Yet, though no serious student would any longer listen to such arguments, it takes a long time before theories that were maintained for a time by serious students, and were then surrendered by them, can be completely eradicated. I shall not touch to-day on the hackneyed question of the "home of the Aryans" except as a warning. There are two quite distinct questions concerning the home of the Aryans.

When students of philology speak of Aryans, they mean by Âryas nothing but people speaking an Aryan language. They affirm nothing about skulls, skins, hair, and all the rest. Ârya with them means speakers of an Aryan language. When, on the contrary, students of physiology speak of dolichocephalic, orthognathic, euthycomic people, they speak of their physiological characteristics only, and affirm nothing whatever about language.

It is clear, therefore, that the home of the Âryas, in the proper sense of that word, can be determined by linguistic evidence only, while the home of a blue-eyed, blond-haired, long-skulled, fair-skinned people can be determined by physiological evidence

only. Any kind of concession or compromise on either side is simply fatal, and has led to nothing but a promiscuous slaughter of innocents. Separate the two armies, and the whole physiological evidence collected by D'Omalius d'Halloy, Latham, and their followers will not fill more than an octavo page; while the linguistic evidence collected by Benfey and his followers will not amount to more than a few words. Everything else is mere rhetoric.

The physiologist is grateful, no doubt, for any additional skull whose historical antecedents can be firmly established; the philologist is grateful for any additional word that can help to indicate the historical or geographical whereabouts of the unknown speakers of Aryan speech. On these points it is possible to argue. They alone have a really scientific value in the eyes of a scholar, because, if there is any difference of opinion on them, it is possible to come to an agreement. As soon, however, as we go beyond these mere matters of fact, which have been collected by real students, everything becomes at once mere vanity and vexation of spirit. I know the appeals that have been made for concessions and some kind of compromise between physiology and philology; but honest students know that on scientific subjects no compromise is admissible. With regard to the home of the Âryas, no honest philologist will allow himself to be driven one step beyond the statement that the unknown people who spoke Aryan languages were, at one time, and before their final separation, settled somewhere in Asia. That may seem very small comfort, but for the present it is all that we have a right to say. Even this must be taken with the limitations which, as all true scholars know, apply to speculations concerning what may have happened, say, five thousand or ten thousand years ago. As to the colour of the skin, the hair, the eyes of those unknown speakers of Aryan speech, the scholar says nothing; and when he speaks of their blood he knows that such a word can be taken in a metaphorical sense only. If we once step from the narrow domain of science into the vast wilderness of mere assertion, then it does not matter what we say. We may say, with Penka, that all Âryas are dolichocephalic, blue-eyed, and blond, or we may say, with Piétrement, that all Âryas are brachycephalic, with brown eyes and black hair (V. d. Gheyn, 1889, p. 26). There is no difference between the two assertions. They are both perfectly unmeaning. They are *vox et prœterea nihil.*

My experiences during the last forty years have only served to confirm the opinion which I expressed forty years ago, that there ought to be a complete separation between philology and physiology. And yet, if I were asked whether such a divorce should now be made absolute, I should say, No. There have been so many unexpected discoveries of new facts, and so many surprising combinations of old facts, that we must always be prepared to hear some new evidence, if only that evidence is brought forward according to the rules which govern the court of true science. It may be that in time the classification of skulls, hair, eyes, and skin may be brought into harmony with the classification of language. We may even go so far as to admit, as a postulate, that the two must have run parallel, at least in the beginning of all things. But with the evidence before us at present, mere wrangling, mere iteration of exploded assertions, mere contradictions, will produce no effect on the true jury, which hardly ever consists of more than twelve trusty men, but with whom the final verdict rests. The very things that most catch the popular ear will by them be ruled out of court. But every single new word, common to all the Aryan languages, and telling of some climatic, geographical, historical, or physiological circumstance in the earliest life of the speakers of Aryan speech, will be truly welcome to philologists quite as much as a skull from an early geological stratum is to the physiologist, and both to the anthropologist, in the widest sense of that name.

But, if all this is so, if the alliance between philology and physiology has hitherto done nothing but mischief, what right, it may be asked, had I to accept the honour of presiding over this Section of Anthropology? If you will allow me to occupy your valuable time a little longer, I shall explain, as shortly as possible, why I thought that I, as a philologist, might do some small amount of good as President of the Anthropological Section.

In spite of all that I have said against the unholy alliance between physiology and philology, I have felt for years—and I believe I am now supported in my opinion by all competent anthropologists—that a knowledge of languages must be considered in future as a *sine quâ non* for every anthropologist.

Anthropology, as you know, has increased so rapidly that it seems to say now, "*Nihil humani a me alienum puto.*" So long as anthropology treated only of the anatomy of the human body, any surgeon might have become an excellent anthropologist. But now, when anthropology includes the study of the earliest thoughts of man, his customs, his laws, his traditions, his legends, his religions, ay, even his early philosophies, a student of anthropology without an accurate knowledge of languages, without the conscience of a scholar, is like a sailor without a compass.

No one disputes this with regard to nations who possess a literature. No one would listen to a man describing the peculiarities of the Greek, the Roman, the Jew, the Arab, the Chinese, without knowing their languages, and being capable of reading the master-works of their literature. We know how often men who have devoted the whole of their life to the study, for instance, of Hebrew, differ, not only as to the meaning of certain words and passages, but as to the very character of the Jews. One authority states that the Jews, and not only the Jews, but all Semitic nations, were possessed of a monotheistic instinct. Another authority shows that all Semitic nations, not excluding the Jews, were polytheistic in their religion, and that the Jehovah of the Jews was not conceived at first as the Supreme Deity, but as a national god only, as the God of the Jews, who, according to the latest view, was originally a fetish or a totem, like all other gods.

You know how widely classical scholars differ on the character of Greeks and Romans, on the meaning of their customs, the purpose of their religious ceremonies—nay, the very essence of their gods. And yet there was a time, not very long ago, when anthropologists would rely on the descriptions of casual travellers, who, after spending a few weeks, or even a few years, among tribes whose language was utterly unknown to them, gave the most marvellous accounts of their customs, their laws, and even of their religion. It may be said that anybody can describe what he sees, even though unable to converse with the people. I say, Decidedly no; and I am supported in this opinion by the most competent judges. Dr. Codrington, who has just published his excellent book on the "Melanesians: their Anthropology and Folk-lore," spent twenty-four years among the Melanesians, learning their dialects, collecting their legends, and making a systematic study of their laws, customs, and superstitions. But what does he say in his preface? "I have felt the truth," he says, "of what Mr. Fison, late missionary in Fiji, has written: 'When a European has been living for two or three years among savages, he is sure to be fully convinced that he knows all about them; when he has been ten years or so amongst them, if he be an observant man, he knows that he knows very little about them, and so begins to learn.'"

How few of the books in which we trust with regard to the characteristic peculiarities of savage races have been written by men who have lived among them for ten or twenty years, and who have learnt their languages till they could speak them as well as the natives themselves.

It is no excuse to say that any traveller who has eyes to see and ears to hear can form a correct estimate of the doings and sayings of savage tribes. It is not so, and anthropologists know from sad experience that it is not so. Suppose a traveller came to a camp where he saw thousands of men and women dancing round the image of a young bull. Suppose that the dancers were all stark naked, that after a time they began to fight, and that at the end of their orgies there were three thousand corpses lying about weltering in their blood. Would not a casual traveller have described such savages as worse than the Negroes of Dahomey? Yet these savages were really the Jews, the chosen people of God. The image was the golden calf, the priest was Aaron, and the chief who ordered the massacre was Moses. We may read the 32nd chapter of Exodus in a very different sense. A traveller who could have conversed with Aaron and Moses might have understood the causes of the revolt and the necessity of the massacre. But without this power of interrogation and mutual explanation, no travellers, however graphic and amusing their stories may be, can be trusted; no statements of theirs can be used by the anthropologist for truly scientific purposes.

From the day when this fact was recognized by the highest authorities in anthropology, and was sanctioned by some at least of our Anthropological, Ethnological, and Folk-lore Societies, a new epoch began, and philology received its right place as the handmaid of anthropology. The most important paragraph in our new charter was this, that in future no one is to be quoted

or relied on as an authority on the customs, traditions, and more particularly on the religious ideas of uncivilized races who has not acquired an acquaintance with their language, sufficient to enable him to converse with them freely on these difficult subjects.

No one would object to this rule when we have to deal with civilized and literary nations. But the languages of Africa, America, Polynesia, and even Australia, are now being studied as formerly Greek, Latin, Hebrew, and Sanskrit only were studied. You have only to compare the promiscuous descriptions of the Hottentots in the works of the best ethnologists with the researches of a real Hottentot scholar like Dr. Hahn to see the advance that has been made. When we read the books of Bishop Callaway on the Zulu, of William Gill and Edward Tregear on the Polynesians, of Horatio Hale on some of the North American races, we feel at once that we are in safe hands, in the hands of real scholars. Even then we must, of course, remember that their knowledge of the languages cannot compare with that of Bentley, or Hermann, or Burnouf, or Ewald. Yet we feel that we cannot go altogether wrong in trusting to their guidance.

I venture to go even a step further, and I believe the time will come when no anthropologist will venture to write on anything concerning the inner life of man without having himself acquired a knowledge of the language in which that inner life finds its truest expression.

This may seem to be exacting too much, but you have only to look, for instance, at the description given of the customs, the laws, the legends, and the religious convictions of the people of India about a hundred years ago, and before Sanskrit began to be studied, and you will be amazed at the utter caricature that is often given there of the intellectual state of the Brahmans compared with what we know of it now from their own literature.

And if that is the case with a people like the Indians, who are a civilized race, possessed of an ancient literature, and well within the focus of history for the last two thousand years, what can be expected in the case of really savage races? One can hardly trust one's eyes when one sees the evidence placed before us by men whose good faith cannot be questioned, and who nevertheless contradict each other flatly on the most ordinary subjects. We owe to one of our Secretaries, Mr. Roth, a most careful collection of all that has been said on the Tasmanians by eye-witnesses. Not the least valuable part of this collection is that it opens our eyes to the utter untrustworthiness of the evidence on which the anthropologist has so often had to rely. In an article on Mr. Roth's book in NATURE, I tried to show that there is not one essential feature in the religion of the Tasmanians on which different authorities have not made assertions diametrically opposed to each other. Some say that the Tasmanians have no idea of a Supreme Being, no rites or ceremonies; others call their religion Dualism, a worship of good and evil spirits. Some maintain that they had deified the powers of Nature, others that they were Devil-worshippers. Some declare their religion to be pure monotheism, combined with belief in the immortality of the soul, the efficacy of prayers and charms. Nay, even the most recent article of faith—the descent of man from some kind of animal—has received a religious sanction among the Tasmanians. For Mr. Horton, who is not given to joking, tells us that they believed "they were originally formed with tails, and without knee-joints, by a benevolent being, and that another descended from heaven, and, compassionating the sufferers, cut off their tails, and with grease softened their knees."

I would undertake to show that what applies to the descriptions given us of the now extinct race of the Tasmanians applies with equal force to the descriptions of almost all the savage races with whom anthropologists have to deal. In the case of large tribes, such as the inhabitants of Australia, the contradictory evidence may, no doubt, be accounted for by the fact that the observations were made in different localities. But the chief reason is always the same—ignorance of the language, and therefore want of sympathy and impossibility of mutual explanation and correction.

Let me, in conclusion, give you one of the most flagrant instances of how a whole race can be totally misrepresented by men ignorant of their language, and how these misrepresentations are at once removed if travellers acquire a knowledge of the language, and thus have not only eyes to see, but ears to hear, tongues to speak, and hearts to feel.

No race has been so cruelly maligned for centuries as the inhabitants of the Andaman Islands. An Arab writer of the ninth century states that their complexion was frightful, their hair frizzled, their countenance and eyes terrible, their feet very large, and almost a cubit in length, and that they go quite naked. Marco Polo (about 1285) declared that the inhabitants are no better than wild beasts, and he goes on to say: "I assure you all the men of this island of Angamanain have heads like dogs, and teeth and eyes likewise; in fact, in the face they are just like big mastiff dogs."

So long as no one could be found to study their language, there was no appeal from these libels. But when, after the Sepoy mutiny in 1857, it was necessary to find a habitation for a large number of convicts, the Andaman Islands, which had already served as a penal settlement on a smaller scale, became a large penal colony under English officers. The havoc that was wrought by this sudden contact between the Andaman Islanders and these civilized Indian convicts was terrible, and the end will probably be the same as in Tasmania—the native population will die out. Fortunately one of the English officers (Mr. Edward Horace Man) did not shrink from the trouble of learning the language spoken by these islanders, and, being a careful observer and perfectly trustworthy, he has given us some accounts of the Andaman aborigines which are real masterpieces of anthropological research. If these islanders must be swept away from the face of the earth, they will now, at all events, leave a good name behind them. Even their outward appearance seems to become different in the eyes of a sympathizing observer from what it was to casual travellers. They are, no doubt, a very small race, their average height being 4 feet 10¾ inches. But this is almost the only charge brought against them which Mr. Man has not been able to rebut. Their hair, he says, is fine, very closely curled, and frizzly. Their colour is dark, but not absolutely black. Their features possess little of the most marked and coarser peculiarities of the Negro type. The projecting jaws, the prominent thick lips, the broad and flattened nose of the genuine Negro, are so softened down as scarcely to be recognized.

But let us hear now what Mr. Man has to tell us about the social, moral, and intellectual qualities of these so-called savages, who had been represented to us as cannibals; as ignorant of the existence of a deity; as knowing no marriage; except what by a bold euphemism has been called communal marriage; as unacquainted with fire; as no better than wild beasts, having heads, teeth, and eyes like dogs—being, in fact, like big mastiffs.

"Before the introduction into the islands of what is called European civilization, the inhabitants," Mr. Man writes, "lived in small villages, their dwellings built of branches and leaves of trees. They were ignorant of agriculture, and kept no poultry or domestic animals. Their pottery was hand-made, their clothing very scanty. They were expert swimmers and divers, and able to manufacture well-made dug-out canoes and outriggers. They were ignorant of metals, ignorant, we are told, of producing fire, though they kept a constant supply of burning and smouldering wood. They made use of shells for their tools, had stone hammers and anvils, bows and arrows, harpoons for killing turtle and fish. Such is the fertility of the island that they have abundance and variety of food all the year round. Their food was invariably cooked, they drank nothing but water, and they did not smoke. People may call this a savage life. I know many a starving labourer who would gladly exchange the benefits of European civilization for the blessings of such savagery."

These small islanders, who have always been represented by a certain class of anthropologists as the lowest stratum of humanity, need not fear comparison, so far as their social life is concerned, with races who are called civilized. So far from being addicted to what is called by the self-contradictory name of communal marriage, Mr. Man tells us that bigamy, polygamy, polyandry, and divorce are unknown to them, and that the marriage contract, so far from being regarded as a merely temporary contract, to be set aside on account of incompatibility of temper or other such causes, is never dissolved. Conjugal fidelity till death is not the exception but the rule, and matrimonial differences, which occur but rarely, are easily settled with or without the intervention of friends. One of the most striking features of their social relations is the marked equality and affection which exist between husband and wife, and the consideration and respect with which women are treated might, with advantage, be emulated by certain classes in our own land. As to cannibalism or infanticide, they are never practised by them.

It is easy to say that Mr. Man may be prejudiced in favour of these little savages, whose language he has been at so much pains to learn. Fortunately, however, all his statements have lately been confirmed by another authority, Colonel Cadell—the Chief Commissioner of these islands. He is a Victoria Cross man, and not likely to be given to over-much sentimentality. Well, this is what he says of these fierce mastiffs, with feet a cubit in length :—

"They are merry little people," he says. "One could not imagine how taking they were. Everyone who had to do with them fell in love with them [these fierce mastiffs]. Contact with civilization had not improved the morality of the natives, but in their natural state they were truthful and honest, generous and self-denying. He had watched them sitting over their fires cooking their evening meal, and it was quite pleasant to notice the absence of greed and the politeness with which they picked off the tit-bits and thrust them into each other's mouths. The forest and sea abundantly supplied their wants, and it was therefore not surprising that the attempts to induce them to take to cultivation had been quite unsuccessful, highly though they appreciated the rice and Indian corn which were occasionally supplied to them. All was grist that came to their mill in the shape of food. The forest supplied them with edible roots and fruits. Bats, rats, flying foxes, iguanas, sea-snakes, mollusks, wild pig, fish, turtle, and last, though not least, the larvæ of beetles, formed welcome additions to their larder. He remembered one morning landing by chance at an encampment of theirs, under the shade of a gigantic forest tree. On one fire was the shell of a turtle, acting as its own pot, in which was simmering the green fat delicious to more educated palates ; on another its flesh was being broiled, together with some splendid fish ; on a third a wild pig was being roasted, its drippings falling on wild yams, and a jar of honey stood close by, all delicacies fit for an alderman's table."

These are things which we might suppose anybody who has eyes to see, and who is not wilfully blind, might have observed. But when we come to traditions, laws, and particularly to religion, no one ought to be listened to as an authority who cannot converse with the natives. For a long time the Mincopies have been represented as without any religion, without even an idea of the Godhead. This opinion received the support of Sir John Lubbock, and has been often repeated without ever having been re-examined. As soon, however, as these Mincopies began to be studied more carefully—more particularly as soon as some persons resident among them had acquired a knowledge of their language, and thereby a means of real communication—their religion came out as clear as daylight. According to Mr. E. H. Man, they have a name for God—*Pûluga*. And how can a race be said to be without a knowledge of God if they have a name for God? *Pûluga* has a very mythological character. He has a stone house in the sky ; he has a wife, whom he created himself, and from whom he has a large family, all, except the eldest, being girls. The mother is supposed to be green (the earth ?), the daughters black ; they are the spirits, called *Môrowin* ; his son is called *Pijchor*. He alone is permitted to live with his father, and to convey his orders to the *Môrowin*. But *Pûluga* was a moral character also. His appearance is like fire, though nowadays he has become invisible. He was never born, and is immortal. The whole world was created by him, except only the powers of evil. He is omniscient, knowing even the thoughts of the heart. He is angered by the commission of certain sins—some very trivial, at least to our mind—but he is pitiful to all who are in distress. He is the judge from whom each soul receives its sentence after death.

According to other authorities, some Andamanese look on the sun as the fountain of all that is good, the moon as a minor power ; and they believe in a number of inferior spirits, the spirits of the forest, the water, and the mountain, as agents of the two higher powers. They believe in an evil spirit also, who seems to have been originally the spirit of the storm. Him they try to pacify by songs, or to frighten away with their arrows.

I suppose I need say no more to show how indispensable a study of language is to every student of anthropology. If anthropology is to maintain its high position as a real science, its alliance with linguistic studies cannot be too close. Its weakest points have always been those where it trusted to the statements of authorities ignorant of language and of the science of language. Its greatest triumphs have been achieved by men such as Dr. Hahn, Bishops Callaway and Colenso, Dr. W. Gill, and last, not least, Mr. Man, who have combined the minute accuracy of the scholar with the comprehensive grasp of the anthropologist, and were thus enabled to use the key of language to unlock the perplexities of savage customs, savage laws and legends, and, particularly, of savage religions and mythologies. If this alliance between anthropology and philology becomes real, then, and then only, may we hope to see Bunsen's prophecy fulfilled, that anthropology will become the highest branch of that science for which this British Association is instituted.

Allow me in conclusion once more to quote some prophetic words from the address which Bunsen delivered before our Section in 1847 :—

"If man is the apex of the creation, it seems right, on the one side, that a historical inquiry into his origin and development should never be allowed to sever itself from the general body of natural science, and in particular from physiology. But, on the other side, if man is the apex of the creation, if he is the end to which all organic formations tend from the very beginning, if man is at once the mystery and the key of natural science, if that is the only view of natural science worthy of our age, then ethnological philology (I should prefer to say anthropology), once established on principles as clear as the physiological are, is the highest branch of that science for the advancement of which this Association is instituted. It is not an appendix to physiology or to anything else ; but its object is, on the contrary, capable of becoming the end and goal of the labours and transactions of a scientific Association."

Much has been achieved by anthropology to justify these hopes and fulfil the prophecies of my old friend Bunsen. Few men live to see the fulfilment of their own prophecies, but they leave disciples whose duty it is to keep their memory alive, and thus to preserve that vital continuity of human knowledge which alone enables us to see in the advancement of all science the historical evolution of eternal truth.

ELECTRICAL STANDARDS.

THE Queen's Printers are now issuing the Report (dated July 23, 1891) to the President of the Board of Trade, of the Committee appointed to consider the question of constructing standards for the measurement of electricity. The Committee included Mr. Courtenay Boyle, C.B., Major P. Cardew, R.E., Mr. E. Graves, Mr. W. H. Preece, F.R.S., Sir W. Thomson, F.R.S., Lord Rayleigh, F.R.S., Prof. G. Carey Foster, F.R.S., Mr. R. T. Glazebrook, F.R.S., Dr. John Hopkinson, F.R.S., Prof. W. E. Ayrton, F.R.S.

In response to an invitation, the following gentlemen attended and gave evidence :—On behalf of the Association of Chambers of Commerce, Mr. Thomas Parker and Mr. Hugh Erat Harrison ; on behalf of the London Council, Prof. Silvanus Thompson ; on behalf of the London Chamber of Commerce, Mr. R. E. Crompton. The Committee were indebted to Dr. J. A. Fleming and Dr. A. Muirhead for valuable information and assistance ; and they state that they had the advantage of the experience and advice of Mr. H. J. Chaney, the Superintendent of Weights and Measures. The Secretary to the Committee was Sir T. W. P. Blomefield, Bart.

The following are the resolutions of the Committee :—

Resolutions.

(1) That it is desirable that new denominations of standards for the measurement of electricity should be made and approved by Her Majesty in Council as Board of Trade standards.

(2) That the magnitudes of these standards should be determined on the electro-magnetic system of measurement with reference to the centimetre as unit of length, the gramme as unit of mass, and the second as unit of time, and that by the terms centimetre and gramme are meant the standards of those denominations deposited with the Board of Trade.

(3) That the standard of electrical resistance should be denominated the ohm, and should have the value 1,000,000,000 in terms of the centimetre and second.

(4) That the resistance offered to an unvarying electric current

by a column of mercury of a constant cross sectional area of 1 square millimetre, and of a length of 106·3 centimetres at the temperature of melting ice may be adopted as 1 ohm.

(5) That the value of the standard of resistance constructed by a committee of the British Association for the Advancement of Science in the years 1863 and 1864, and known as the British Association unit, may be taken as ·9866 of the ohm.

(6) That a material standard, constructed in solid metal, and verified by comparison with the British Association unit, should be adopted as the standard ohm.

(7) That for the purpose of replacing the standard, if lost, destroyed, or damaged, and for ordinary use, a limited number of copies should be constructed, which should be periodically compared with the standard ohm and with the British Association unit.

(8) That resistances constructed in solid metal should be adopted as Board of Trade standards for multiples and sub-multiples of the ohm.

(9) That the standard of electrical current should be denominated the ampere, and should have the value one-tenth (0·1) in terms of the centimetre, gramme, and second.

(10) That an unvarying current which, when passed through a solution of nitrate of silver in water, in accordance with the specification attached to this report, deposits silver at the rate of 0·001118 of a gramme per second, may be taken as a current of 1 ampere.

(11) That an alternating current of 1 ampere shall mean a current such that the square root of the time-average of the square of its strength at each instant in amperes is unity.

(12) That instruments constructed on the principle of the balance, in which by the proper disposition of the conductors, forces of attraction and repulsion are produced, which depend upon the amount of current passing, and are balanced by known weights, should be adopted as the Board of Trade standards for the measurement of current, whether unvarying or alternating.

(13) That the standard of electrical pressure should be denominated the volt, being the pressure which, if steadily applied to a conductor whose resistance is 1 ohm, will produce a current of 1 ampere.

(14) That the electrical pressure at a temperature of 62° F. between the poles or electrodes of the voltaic cell known as Clark's cell, may be taken as not differing from a pressure of 1·433 volts, by more than an amount which will be determined by a sub-committee appointed to investigate the question, who will prepare a specification for the construction and use of the cell.

(15) That an alternating pressure of 1 volt shall mean a pressure such that the square root of the time-average of the square of its value at each instant in volts is unity.

(16) That instruments constructed on the principle of Sir W. Thomson's quadrant electrometer used idiostatically, and for high-pressure instruments on the principle of the balance, electrostatic forces being balanced against a known weight, should be adopted as Board of Trade standards for the measurement of pressure, whether unvarying or alternating.

We have adopted the system of electrical units originally defined by the British Association for the Advancement of Science, and we have found in its recent researches, as well as in the deliberations of the International Congress on Electrical Units, held in Paris, valuable guidance for determining the exact magnitudes of the several units of electrical measurement, as well as for the verification of the material standards.

We have stated the relation between the proposed standard ohm and the unit of resistance originally determined by the British Association, and have also stated its relation to the mercurial standard adopted by the International Conference.

We find that considerations of practical importance make it undesirable to adopt a mercurial standard, we have, therefore, preferred to adopt a material standard constructed in solid metal.

It appears to us to be necessary that in transactions between buyer and seller, a legal character should henceforth be assigned to the units of electrical measurement now suggested, and with this view, that the issue of an Order in Council should be recommended, under the Weights and Measures Act, in the form annexed to this report.

Specification referred to in Resolution 10.

In the following specification the term silver voltameter means the arrangement of apparatus by means of which an electric current is passed through a solution of nitrate of silver in water. The silver voltameter measures the total electrical quantity which has passed during the time of the experiment, and by noting this time the time-average of the current, or if the current has been kept constant, the current itself, can be deduced.

In employing the silver voltameter to measure currents of about 1 ampere, the following arrangements should be adopted. The kathode on which the silver is to be deposited should take the form of a platinum bowl not less than 10 cm. in diameter, and from 4 to 5 cm. in depth.

The anode should be a plate of pure silver some 30 square cm. in area and 2 or 3 millimetres in thickness.

This is supported horizontally in the liquid near the top of the solution by a platinum wire passed through holes in the plate at opposite corners. To prevent the disintegrated silver which is formed on the anode from falling on to the kathode, the anode should be wrapped round with pure filter paper, secured at the back with sealing-wax.

The liquid should consist of a neutral solution of pure silver nitrate, containing about 15 parts by weight of the nitrate to 85 parts of water.

The resistance of the voltameter changes somewhat as the current passes. To prevent these changes having too great an effect on the current, some resistance besides that of the voltameter should be inserted in the circuit. The total metallic resistance of the circuit should not be less than 10 ohms.

Method of making a Measurement.—The platinum bowl is washed with nitric acid and distilled water, dried by heat, and then left to cool in a desiccator. When thoroughly dry, it is weighed carefully.

It is nearly filled with the solution, and connected to the rest of the circuit by being placed on a clean copper support, to which a binding screw is attached. This copper support must be insulated.

The anode is then immersed in the solution, so as to be well covered by it, and supported in that position; the connections to the rest of the circuit are made.

Contact is made at the key, noting the time of contact. The current is allowed to pass for not less than half an hour, and the time at which contact is broken is observed. Care must be taken that the clock used is keeping correct time during this interval.

The solution is now removed from the bowl, and the deposit is washed with distilled water and left to soak for at least six hours. It is then rinsed successively with distilled water and absolute alcohol, and dried in a hot-air bath at a temperature of about 160° C. After cooling in a desiccator, it is weighed again. The gain in weight gives the silver deposited.

To find the current in amperes, this weight, expressed in grammes, must be divided by the number of seconds during which the current has been passed, and by 0·001118.

The result will be the time-average of the current, if during the interval the current has varied.

In determining by this method the constant of an instrument the current should be kept as nearly constant as possible, and the readings of the instrument taken at frequent observed intervals of time. These observations give a curve from which the reading corresponding to the mean current (time-average of the current) can be found. The current, as calculated by the voltameter, corresponds to this reading.

NOTES.

THE International Meteorological Conference at Munich was opened on August 26. Dr. C. Lang, Director of the Bavarian Meteorological Service, was unanimously elected President. Prof. M. W. Harrington (Chief of the United States Weather Bureau) and Prof. E. Mascart (Director of the French Meteorological Service) were elected Vice-Presidents. Mr. R. H. Scott (Secretary of the Meteorological Office), Dr. F. Erk (Munich), and M. L. Teisserenc de Bort (Paris) were elected Secretaries. Thirty members were present, including representatives from Brazil, Queensland, and the United States. We hope in a future number to give some account of the proceedings.

DR. BARCLAY, whose death at Simla has been announced, was working on the Leprosy Commission, and his loss is

described by the Indian press as not only a severe one to India, but for the whole scientific world. His special study was cryptogamic botany. He made important researches in diseases of Indian plants, and had gained a continental reputation. Several of his papers were published in the Linnean Society's Transactions. His great ambition was to solve Indian wheat disease, and he was to have studied coffee disease in Southern India next winter.

PARTLY owing to Dr. Barclay's death, the Indian Leprosy Report will be delayed a short time. The practical work is virtually completed, and the Draft Report for the Government of India is in type. The chief work now consists in correcting the proofs and the preparation of the plates, maps, and statistics. On the two main questions with which they were to deal, viz. the contagiousness and hereditary transmission of the disease, the Commission have come to a unanimous decision, but their conclusions will not be known till the Report is published by the National Leprosy Fund.

THE statutory ninth meeting of the International Congress of Orientalists began in the hall of the Inner Temple on Tuesday, when an address was delivered by the Master of St. John's College, Cambridge.

AN election to the Coutts Trotter Studentship, at Trinity College, Cambridge, will take place next month. Applications from candidates must be sent in to the College office, addressed to the Secretary of the Coutts Trotter Studentship Committee, on or before October 15. The studentship is tenable for two years, and is for original research in physiology or in physics.

WE are glad to learn that a number of the friends of the late Mr. N. R. Pogson are thinking of raising a memorial to him in Madras.

WITH reference to a recent note, we learn from New South Wales that the Minister for Mines and Agriculture (the Hon. Sydney Smith) has appointed Mr. Niel Harper, formerly a dairy farmer of excellent repute in the South Coast District, to take charge of the travelling dairy, which is to be sent to the different districts of the colony under the control of the Department of Agriculture. It will be necessary for the Agricultural Society, or a local Committee, to provide the requirements of the dairy such as a building suitable for its operations, and giving accommodation sufficient for ten pupils, who will be thoroughly instructed in all dairying operations. Also, for the carriage of the plant to and from the nearest railway station or wharf to the scene of operations, together with the necessary labour to assist in the rough work of cleaning up, &c. The Society, or Committee, will need to provide also a sufficient supply of milk, say about fifty gallons daily, for the operations of the dairy, and plenty of clean water for washing butter and cleaning up. Each Society, or Committee, undertaking to furnish these requirements will be entitled to nominate at least ten pupils (either male or female) for the full course of instruction in dairy operations, who will afterwards be examined with a view to receiving a dairy certificate in the event of their showing a satisfactory knowledge of the course of instruction. Of course the general public will be admitted to see all the operations of the dairy, which will work for ten days at each place where set up. All district Societies and Committees desiring to have the benefit of this course of instruction for their localities should make early application to the Director of Agriculture, from whom regulations and instructions can be obtained. Is our Minister of Agriculture doing anything similar?

AT the request of the Russian Ambassador in London, the Secretary of State for India has asked the Government of India to afford facilities to Prof. Tichomiroff, who is about to visit certain parts of India, Ceylon, and China, with the view of studying the administration of botanical gardens and cinchona plantations, and to M. Gondatti, who is about to study tea and silkworm cultivation in India, Ceylon, and China.

CAPTAIN WAHAB, R.E., will have charge of a party which is to make a survey of the country round Aden during the coming winter.

MR. GRIESBACH, of the Geological Survey of India, has proceeded with a survey party to Upper Burmah, where he will remain about two years to examine thoroughly the geological condition of the country.

AN important resolution of the Government of India on the reorganization of the superior staff of the Indian Forest Department has been issued. At an extra yearly cost of three lakhs of rupees, the Imperial and Provincial Services are to be separated. The Imperial is to be recruited solely under covenant with the Secretary of State, and the average pay raised 6 per cent. The Provincial Service gives 126 appointments, up to 600 rupees a month, to natives of India. The Forest Department is the first to introduce a complete scheme under the Public Service Commission.

NINE members of the *Kite* Arctic Expedition arrived at Halifax, N.S., on August 30. The Expedition reached 77° 43' N., and 70° 20' W. They have brought with them immense collections of flowers, herbs, and butterflies, some of which were previously unknown. It is stated that "they found all the published charts of Greenland to be incorrect."

EXPERIMENTS for the production of artificial rain are now being made in Texas. They are conducted by members of the Signal Corps, acting under the direction of the Minister of Agriculture, and have been undertaken in accordance with a vote of the United States Congress. Adequate reports on the subject have not yet reached this country, but it is claimed that the experiments have been attended by remarkable success.

MR. GEORGE FORBES, writing to the *Times* on August 31, gave the following account of a meteor which he had seen at Maidenhead on the previous evening at 8h. 22m.:—"It was brighter than Jupiter when I first saw it; it lasted three seconds from the time I first saw it, steadily increasing in size and brightness, becoming pear-shaped, and blue showing in its rear part when at its brightest—*i.e.* just before extinction. There was no train, the luminosity not extending more than 1° behind it. At the end it became intensely bright, and then disappeared suddenly. It passed a little south of α Cassiopeiæ, and also a little south of γ Andromedæ. I first saw it at 1h. 45m. R.A. and 50° N. Decl., and it ended at 2h. 0m. R.A. and 39° N. Decl."

IN the *Meteorologische Zeitschrift* for July, Prof. H. Mohn discusses the present methods of reduction of meteorological observations; after the completion of twenty-five years of observations at the Norwegian stations, he has decided upon making certain more or less important alterations, commencing from January 1 last. (1) As regards pressure, to introduce the correction for standard gravity at sea-level, in latitude 45°, which amounts to 0·16 inch between the equator and the Poles, and to as much as 0·03 inch between two extreme stations of the Norwegian system. And to apply a correction due to diurnal range (to be determined from hourly observations) to the monthly means obtained and published from two or three observations daily. (2) Similarly, for temperature and humidity, to apply corrections to the published monthly values obtained from two or three daily observations. He fully explains the methods he has adopted for obtaining the corrections to be applied, and we think the matter is worthy of the attention of

meteorologists who publish their results. Prof. W. von Bezold gives an interesting summary of his paper on the theory of cyclones, which he laid before the Berlin Academy in December last, and in which he treated of the more recent views regarding the laws of atmospheric circulation; he also refers to various points which have to be dealt with for the further advancement of the science.

M. LANCASTER has recently indicated in *Ciel et Terre* the divergences from normal temperature in Europe in the five years 1886-90. It appears (and is shown in a map) that the centre of the "island of cold" lies over the north of France, the south of Belgium, and the most western parts of Germany. From this centre the cold decreases pretty regularly outwards on all sides to a nearly circular line of *nil* divergence, which, embracing the whole of Great Britain, crosses the south of Sweden, then goes along the German-Russian frontier, through Hungary, the south of Italy, the north of Africa, and across Spain. Throughout this inclosed region abnormally low temperatures have prevailed. Siberia, too, shows thermal depression, which M. Lancaster thinks may be connected with that in Western Europe.

SR. H. MORIZE, astronomer at the Observatory of Rio de Janeiro, has just published a "Sketch of the Climatology of Brazil," which will be welcome to meteorologists, as hitherto systematic observations have only been published for a very few points of that immense country, covering 39° of latitude. The present sketch has been drawn up mainly from the observations of travellers and private observers. We can only extract a few brief notes. Thunderstorms are very frequent all along the coast, and are mostly harmless; regular cyclones are very rare—the most dangerous winds are the pamperos, which blow from the south-west, and have been fully described by the late Admiral Fitz-Roy, and a still more rare and dangerous wind which blows from the south-east. As regards temperature, the author has divided the country into three zones, and some valuable data are given for various localities. Parts of the country are subject to prolonged drought; it is said that at Pernambuco no rain fell during the whole year 1792, and a third of the population died from its effects; droughts have recurred during the present century with some regularity, the last being in the year 1888-89. The most complete series of observations is that for Rio de Janeiro, which dates from 1781, with occasional interruptions. The highest shade temperature was 99°·5 in November 1883, and the lowest 50°·4 in September 1882. There are also good series of observations for Rio Grande do Sul and São-Paulo.

ONE of the most important contributions made of late years to our knowledge of the embryology of flowering plants is to be found in a paper by a lady, Mdlle. C. Sokolowa, in the *Bulletin* of the Imperial Society of Naturalists of Moscow. It relates especially to the formation of the endosperm within the embryo-sac of Gymnosperms, the particulars of which are described in great detail. The process is somewhat intermediate between that of ordinary cell-division and that known as free cell-formation. It is a group of short cells belonging to the parietal layer of this endosperm that ultimately develop into the corpuscles or secondary embryo-sacs, the homologues of the archegones of Vascular Cryptogams. In the tendency displayed by *Pinus* and *Cephalotaxus* towards the early differentiation of these cells, Mdlle. Sokolowa sees the foreshadowing of the process which is universal in Angiosperms, the formation of the embryonic vesicles before that of the endosperm. *Ephedra* exhibits a still closer approximation in this respect to Angiosperms than to the Conifers. In the same number of the *Bulletin* is an interesting and important paper by Prof. Goroschankin on the "Structure and Reproduction of *Chlamydomonas*." The former paper is written in French, the latter in German.

THE survey of the cañon of the Colorado has now been completed, and Mr. R. B. Stanton has given a full account of it in the American *Engineering News*. In spite of the great depths of the cañon and the cliffs of sandstone, marble, and granite composing it, a railway can in his opinion be built through it without much tunnelling, thus opening up some of the grandest scenery of the world. In many places the cañon expands into wide valleys, and even where it narrows there are terraces along the sides like the "parallel roads" of Glen Roy in Scotland, which seem designed by nature for track and rail. The tributaries which enter the cañon laterally are as a rule small, and can be easily bridged. The distance of 1019 miles through the cañon district will only comprise 20 miles of tunnelling and 99 miles of granite cutting.

AT the meeting of the Linnean Society of New South Wales, on June 24, Mr. C. Darley exhibited some very large examples of the shells of the mud oyster (*Ostrea edulis*, var. *angasi*) obtained during dredging operations in Rozelle Bay, Sydney Harbour. They occur in great numbers at a depth of 10 to 12 feet below low water-mark beneath a layer of black mud 3 to 4 feet thick, and are much larger than specimens now to be found living in the harbour. The two valves of one pair weigh 3 pounds 12 ounces, and measure about 8 × 6 inches.

IN *Nature Notes* for August Mr. R. T. Lewis, on the authority of a correspondent in whose trustworthiness he has entire confidence, gives a curious account of the appreciation with which the song of the Cicada is heard by insects other than those of its own genus. The correspondent has frequently observed in Natal that when the Cicada is singing at its loudest, in the hottest portion of the day, it is attended by a number of other insects with lovely, gauze-like, iridescent wings, whose demeanour has left no doubt on his mind that the music is the attraction. The Cicada, when singing, usually stations itself upon the trunk of a tree with its head uppermost, and the insects in question, to the number sometimes of fifteen or sixteen, form themselves into a rough semicircle at a short distance around its head. During a performance one of the insects was observed occasionally to approach the Cicada and to touch it upon its front leg or antennæ, which proceeding was resented by a vigorous stroke of the foot by the Cicada, without, however, any cessation of its song. The insects composing the audience are extremely active; and so wary that they take flight at the least alarm on the too near approach of any intruder. Some of them, however, have been captured; and on examination these "proved to belong to the same family as that most beautiful of British insects—the lace-wing fly, which, indeed, they closely resemble except as to size, their measurement across the expanded wings being a little over two inches; they have since been identified by Mr. Kirby at the British Museum as *Nothochrysa gigantea*."

ACCORDING to a telegram through Dalziel's agency from Vancouver, the Canadian Pacific steamer *Japan*, which arrived there from Hong Kong and Yokohama on August 30, has reported a terrific typhoon at Kobe on the 16th inst. All the steamers in the harbour dragged their anchors, and many native boats were cast ashore and their crews were drowned. A German steamship was driven ashore and eight of the crew were drowned, and an Indian barque *Singlas* was wrecked, and all on board were lost. Her Majesty's gunboat *Tweed* sank. Altogether among natives and foreigners it is believed that 250 lives were lost. The wind did much damage inshore. In one coast town forty-five persons were killed by falling houses.

THE Science and Art Department has issued its Directory (revised to June 1891), with regulations for establishing and conducting science and art schools and classes.

THE University College, Bristol, has issued its Calendar for the session 1891–92. While the College supplies for persons of either sex above the ordinary school age the means of continuing their studies in science, languages, history, and literature, it claims especially to afford appropriate and systematic instruction in those branches of applied science which are more nearly connected with the arts and manufactures.

SIR WILLIAM MACGREGOR, Governor of British New Guinea, recently ascended Mount Yule, or Kovio, as he prefers to call it. The Kovio range is volcanic and isolated from the main chain, of which Mount Owen Stanley is the culmination. The Kovio range is under 11,000 feet high, and is wooded to the very summit. Native tracks lead through the forest to the top of Mount Yule, on the south-west front of which there is a magnificent series of cascades, having a height of 4000 feet in all. A new river and a new lake were also discovered; but the animal life of the region was far from abundant.

THE last Bulletin of the Geographical Society of the United States contains an interesting paper on the curious discovery of human remains under the Tuolumne Table Mountain of California. Bones of men and grinding instruments were there found by Prof. Whitney, embedded in auriferous gravel under lava at the foot of the mountain. Remains of plants belonging to the Tertiary age, and the bones of extinct Mammalia, such as the rhinoceros of the West and the American mastodon, are also met with in the same strata. Pestles, mortars, and broken spear-heads are the most remarkable of the implements discovered.

FROM the last Report of the Council of the North China Asiatic Society of Shanghai we learn that the printers have now in hand a most valuable work by Dr. Bretschneider on the "Botany of the Chinese Classics," the publication of which, on account of its length and technical difficulties, has been much delayed. Some time, however, must yet elapse before it can be issued. Mr. Faber has undertaken the difficult task of correcting the printer's proofs and adding many notes, which will render the work the most comprehensive and useful book which has yet appeared on Chinese botany.

THE new number of the *Internationales Archiv für Ethnographie* (Band iv., Heft 4) opens with an interesting paper by Prof. A. C. Haddon, on the Tugeri head-hunters of New Guinea. Mr. J. J. M. de Groot has an article on the wedding garments of a Chinese woman, and Dr. Julius Jacobs discusses (in Dutch) the ideas of Dr. Ploss on the origin of circumcision.

MESSRS. WEST, NEWMAN, AND CO., have reprinted from the *Journal of Botany* for 1891, a "Key to the Genera and Species of British Mosses," by the Rev. H. G. Jameson. The author explains that his work is not intended to take the place of a more detailed text-book, but merely to serve as a clue by which the student may ascertain in what part of his book he should look for the description of any unknown specimen.

WE have received a Report on Astronomical Observations for 1886, by George H. Boehmer. Directors of observatories, and astronomers generally, are earnestly requested by Mr. Boehmer to criticize his work freely, and to send him such corrections and additions as may seem to them necessary or desirable.

MESSRS. W. WESLEY AND SON have published a catalogue of botanical books which they are offering for sale.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, a Pinche Monkey (*Midas œdipus* ♂) from New Granada, presented by Mr. H. Wather; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Any Jones, F.Z.S.; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Miss Caplen; a Marbled Polychrus (*Polychrus marmoratus*), a Thick-necked Tree-Boa (*Epicrates cenchris*) from Trinidad, presented by Messrs R. R. Mole and F. W. Urich; a —— Salamander (*Amblystoma punctatum*) from North America, presented by Mr. J. H. Thomson; a Smooth Snake (*Coronella lævis*), European, presented by Mr. F. C. Adams; a Great Kangaroo (*Macropus giganteus*), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited.

OUR ASTRONOMICAL COLUMN.

STARS HAVING PECULIAR SPECTRA.—From a communication by Prof. E. C. Pickering to *Astronomische Nachrichten*, No. 3054, it appears that the hydrogen lines G and *h* are bright in a photograph of the spectrum of a third-type star, D.M. + 39° 4851 (R.A. 22h. 24·7m., Decl. + 39° 48′, 1900), taken on July 6. And an examination of the photographs of this region taken on different dates has confirmed the long-period variability of which this spectroscopic appearance is now recognized as a distinctive feature. The seventh magnitude star D.M. − 10° 5057, whose approximate position for 1900 is R.A. 19h. 17·7m., Decl. − 10° 54′, has been previously announced as having a spectrum of the fourth type, but later photographs show that the lines in the spectrum are not those due to hydrogen, but are sometimes seen to be broad bands, and at other times as doubles. These peculiarities, however, cannot be made out in the visible spectrum of the star.

PHOTOGRAPHY OF SOLAR PROMINENCES.—At the meeting of the Paris Academy of Sciences on August 17, M. Deslandres exhibited some of the results he has obtained since May in the photography of bright lines in solar prominence spectra. The negatives exhibit good reversals of H and K, and the first two lines of the ultra-violet hydrogen series. And M. Deslandres finds from a direct comparison with a Giessler tube that the bright line a little less refrangible than H is really due to hydrogen. It is proposed to construct an apparatus by means of which the prominences at all points on the sun's limb may be photographed and their velocities determined. That two observers, Prof. Hale and M. Deslandres, should have been simultaneously working to attain the same object is somewhat remarkable. From the various papers published by the former gentleman, it appears that he obtained the first reversals of H and K in prominence spectra about the middle of April, and the first photograph showing the form of a prominence on May 7.

ENCKE'S COMET (*c* 1891).—The following ephemeris is from one given by Dr. Backlund in the *Bulletin Astronomique* for August:—

Ephemeris for Berlin Midnight.

1891.	R.A. h. m. s.	Decl. ° ′ ″	Log *r*.	Log Δ.
Aug. 28	5 2 29	+35 8 0	0·0563	0·0454
Sept. 1	6 31 22	35 9 5	0·0316	0·0229
„ 5	7 2 24	34 43 5	0·0045	0·0025
„ 9	7 35 36	33 40 9	9·9749	9·9850
„ 13	8 10 25	31 58 4	9·9424	9·9719
„ 17	8 45 49	29 29 7	9·9060	9·9638
„ 21	9 20 59	26 16 9	9·8655	9·9626
„ 25	9 55 0	22 25 7	9·8200	9·9677
„ 29	10 27 27	18 4 7	9·7689	9·9727
Oct. 3	10 58 18	13 22 6	9·7120	9·9983
„ 7	11 27 55	8 27 2	9·6503	0·0223
„ 11	11 57 2	+ 3 23 2	9·5897	0·0498
„ 15	12 26 30	− 1 44 4	9·5744	0·0783
„ 19	12 56 53	6 46 1	9·5336	0·1050
„ 23	13 27 41	11 24 3	9·5634	0·1278
„ 27	13 58 6	15 26 8	9·6187	0·1472
„ 31	14 27 27	−18 49 3	9·6809	0·1646

The comet is now in Auriga, which is in the north-east about 10 p.m. On September 8 it passes about 2° north of Castor.

A NEW ASTEROID (312).—On August 12, Dr. Palisa observed what may be a new asteroid, or, according to Dr. Berberich, it may turn out to be identical with (149) or (291).

JUPITER AND HIS MARKINGS.

DURING the last few years, Jupiter has been situated so far south of the equator that telescopic observations have had to be pursued under all the disadvantages inseparable from viewing an object at a low altitude. But the conditions are now much improved; the planet, though still in south declination, will be some 11° north of his position in 1890, and will therefore remain much longer above the horizon, and present a better defined and larger disk than during the few preceding oppositions, so that the study of his surface-markings may be resumed under very encouraging circumstances.

The great red spot has been visible and its appearance and movements closely watched during thirteen years, for it was in July 1878 that it was first announced as a striking object. But it probably existed long before this, for the drawings of previous observers include forms which have a very suggestive resemblance to the red spot, though they are under a less conspicuous aspect. There is, in fact, little doubt that this marking is an old feature, but it is liable to considerable variations of tint, inducing obvious changes in its general appearance as presented to telescopic observers. Layers of cloud, moving with unequal velocities and at different elevations above the surface of the planet, probably overlap the spot and partially obliterate it at times, but its definite elliptical outline has been always preserved, and its dimensions have not varied materially. It is the colouring of the spot that has exhibited inconstancy, and especially that of the central region, which changed from a brick-red in 1878–81 to a very light tint, differing little, if at all, from the other parts of the planet's disk in the same latitude. But the margin of the spot has been more durable, and it was visible for several years as a pink ellipse, offering a great similarity to the ellipse seen by Gledhill in 1869–70.

After a somewhat precarious existence, the spot appears to be recovering prominence, though its present aspect will not bear comparison with the features it presented about twelve years ago. Still it is now a fairly conspicuous marking, with a depth of tint far more pronounced than in the years 1884–85. The central part of the spot appears to have regained the reddish hue, and the general appearance of the object is sufficiently marked to recall the grand views it afforded at the period of its best display.

The variable motion of the spot has formed one of its most interesting attributes, and I give below a table of the mean rotation-period deduced from observations during the last eleven oppositions of Jupiter:—

Limiting dates.	Rotations.	Period. h.	m.	s.
1879 July 10—1880 Feb. 7	512	9	55	34·2
1880 Sep. 27—1881 Mar. 17	413	9	55	35·6
1881 July 8—1882 Mar. 30	640	9	55	38·2
1882 July 29—1883 May 4	674	9	55	39·1
1883 Aug. 23—1884 June 12	710	9	55	39·1
1884 Sep. 21—1885 July 8	700	9	55	39·2
1885 Oct. 24—1886 July 24	659	9	55	41·1
1886 Nov. 23—1887 Aug. 2	609	9	55	40·5
1888 Feb. 12—1888 Aug. 22	462	9	55	40·2
1889 May 28—1889 Nov. 26	439	9	55	40·0
1890 May 22—1890 Nov. 25	451	9	55	40·2

On August 7, 1891, I re-observed the spot with a 10-inch reflector, power 252, and found it well-defined and fairly conspicuous. It passed the central meridian of the planet at 11h. 32m., so that it followed Marth's zero meridian (System II.) only 3 minutes. This nearly agrees with two observations by Mr. A. S. Williams in May last, which placed the spot 4 minutes behind the zero meridian. Mr. Marth's computations are to be found in the *Monthly Notices* for March 1891, and they supply a valuable guide to all students of Jovian phenomena.

Apart from the red spot, it is desirable that the white spots near the planet's equator, and the similar markings which verge the northern side of the north equatorial belt, should be assiduously followed, and their individual rotation periods ascertained from a number of fresh observations. These markings are severally controlled by proper motions of very irregular character, and some singular alternations of visibility also affect them. Mr. Williams finds that the equatorial white spots have exhibited a great slackening of speed in recent years. This variation apparently affects the entire equatorial zone, and it will be important to determine the exact extent of it, and whether it is sustained in the present year. The changes of velocity alluded to are scarcely progressive in the same direction; we may expect to find an acceleration sooner or later to compensate for the relatively slow movement of the spots in the few past years. It is not unlikely that the various markings show oscillations of speed recurring at uniform intervals.

Students of this interesting planet will find abundance of materials to collate and discuss. There is ample evidence of the reappearance of certain features after periods of non-visibility. Some of the more durable markings apparently suffer temporary obscuration by vaporous masses suspended above them in the Jovian atmosphere. The disposition of the belts is also liable to changes, though not so rapidly as is generally supposed, for many of the alleged variations have been due to differences in telescopic definition or to the rapid rotation of the planet; circumstances which have not always been adequately allowed for.

W. F. DENNING.

SCIENTIFIC SERIALS.

American Journal of Science, August.—Some of the features of non-volcanic igneous ejections, as illustrated in the four "Rocks" of the New Haven region, West Rock, Pine Rock, Mile Rock, and East Rock, by James D. Dana. A few of the conclusions arrived at from the observations recorded in this paper are that igneous eruptions occurred in the New Haven region after the sandstone had been upturned. The liquid rock forced its way between layers of the sandstone, and lifted it up where the pressure of the rock was not too great to prevent the upheaval. This intrusive action was favoured by the fact that the fissure supplying the lava was inclined in the same direction as the layers of the uplifted sandstone. And the foliation of the underlying schists did not determine the course and dip of the supply fissures. The paper is illustrated by several excellent photographs of the formations investigated.—Note on a reconnaissance of the Ouachita mountain system in Indian territory, by Robert T. Hill.—The continuity of solid and liquid, by Carl Barus. By means of the simple arrangement described in this paper, the author is able to obtain at once the isothermals and isopiestics, and therefore the isometrics, both for the solid and liquid states of the substances experimented upon. The relation of solidification and fusion to pressure and the pressure changes of the isothermal specific volumes of solid and liquid at the solidifying and melting points can also be determined. And from such results the character of fusion and the probable position of critical and transitional points can be found. The author has as yet only investigated the behaviour of naphthalene by his method, but the whole work throws considerable light upon the relation of pressure to phenomena of fusion and solution.—Note on the asphaltum of Utah and Colorado, by George H. Stone. The author has visited all the known asphalte fields of Western Colorado and North-Eastern Utah. The observations he has made bear upon the origin of petroleum, asphalte, natural gas, and other subterranean hydrocarbons, but the facts are hardly sufficient to lead to definite conclusions.—Photographic investigation of prominences and their spectra, by George E. Hale. Account is given of the methods employed by the author for the photography of invisible solar prominences. Special attention has been directed to the photography of the bright prominence lines running through H and K, with a slit tangential to the sun's limb. Four reproductions of negatives showing prominences illustrate the paper.—A gold-bearing hot spring deposit, by Walter Harvey Weed. A microscopical and chemical examination of some specimens of ore from the Mount Morgan Gold Mine, Queensland, demonstrates that the mine is a deposit of a hot spring, the ore being a siliceous sinter impregnated with auriferous hæmatite. This is the only hot spring deposit that has been found to contain gold in commercially valuable quantities, and although the sinter deposits from the hot springs of Yellowstone Park resemble those from Mount Morgan, no trace of the precious metals has been found in them.—Restoration of Stegosaurus, by O. C. Marsh. The species restored is *Stegosaurus ungulatus*, from the Upper Jurassic of Wyoming. A plate, representing the reptile one-thirtieth its natural size, accompanies the paper.

THE *American Meteorological Journal* for July contains the following articles:—Franklin's kite experiment, by A. McAdie. After giving various details respecting Franklin's experiments, the author describes similar experiments recently carried on at the Blue Hill Observatory, near Boston, U.S., the chief advance being that at every step the electrical potential of the atmosphere was measured by an electrometer. The kite was sent up on several days, and at a height of 1000 feet sparks over $\frac{1}{8}$ inch in length were obtained; while abnormal movements of the stream of water from the electrometer during electrical disturbance always foretold when a flash of lightning was about to occur.—Cloud heights and velocities at Blue Hill Observatory, by H. H. Clayton. This paper contains the results of cloud observations made at Mr. A. L. Rotch's Observatory during the last five years. The average heights of some of the principal clouds were: *nimbus* 412 metres, *cumulus* (base) 1558 m., false *cirrus* 6500 m., *cirro-stratus* 9652 m., *cirrus* 10,135 m. The cumulus is highest at Blue Hill during the middle of the day. The Upsala observations show that the base of the cumulus, as well as the cirrus, increases in height until evening, but neither of these conclusions apply to the observations at Blue Hill. The average velocity found for the cirrus (82 miles an hour) is twice as great as that found at Upsala. The extreme velocity was found to be 133 miles an hour. A comparison between wind and cloud velocity shows that below 500 metres the wind velocity is less than the cloud velocity. Above that, the excess of the cloud velocity increases up to 1000 metres, and then decreases again till about 1700 metres, after which it steadily increases. This decrease between 1000 and 1700 metres is very probably due to the fact that the clouds between 700 and 1000 metres were mostly observed during the morning, when the cumulus moves most rapidly, and that the clouds between 1000 and 1700 metres were mostly observed during the afternoon, when the cumulus moves slowest.—Meteorological kite-flying, by W. A. Eddy. This is an account of some experiments made at Bergen Point, New Jersey, to determine the vertical extension of warm air currents by means of self-recording thermometers carried by a kite string. Experiments showed that an altitude of 1800 feet could be obtained by using one kite, and that many hundred feet could be added to the altitude by lifting the weight of slack string by fastening on larger kites. It is estimated that by this means an altitude of 4000 feet was obtained. The minimum temperature at an altitude of about 1500 feet, on February 14 last, was only 2° lower than at the surface.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 24.—M. Duchartre in the chair.—Remarks on the dynamic conditions of the development of cometary tails, by Dom Et. Siffert.—*Résumé* of solar observations made at the Observatory of the Roman College during the second quarter of 1891, by M. Tacchini.—On cyclic systems, by A. Ribaucour.—A property of involution, common to a group of five right lines and a system of nine planes, by M. P. Serret.—On the tension of water-vapour up to 200 atmospheres, by M. Ch. Antoine. From the expression $t = \frac{1638 - 0{\cdot}0005\mathrm{P}^2}{5{\cdot}0402 - \log \mathrm{P}} - 225$, deduced from the experimental results of MM. Cailletet and Colardeau, the author deduces formulæ for the calculation of P to a first approximation, by the aid of the general formula $\mathrm{P} = \mathrm{G}\left(\frac{\mathrm{T} - \lambda}{\mathrm{T}}\right)^a$, given by J. Bertrand to express the tension of vapours. The formulæ given are:—

$$\mathrm{P}^1 = [0{\cdot}0058824\,(t + 70)]^{6{\cdot}0} \text{ applicable from } 0^\circ\text{–}100^\circ;$$
$$\mathrm{P}^1 = [0{\cdot}0064516\,(t + 55)]^{5{\cdot}5} \text{ applicable from } 50^\circ\text{–}200^\circ;$$
$$\mathrm{P}^1 = [0{\cdot}0071069\,(t + 41)]^{5{\cdot}0} \text{ applicable from } 220^\circ\text{–}365^\circ.$$

The value P^1 is then used in Cailletet's formula to calculate P, of which tabulated values are given.—On the rejection, by the liver, of bile introduced into the blood, by M. E. Wertheimer. The author has examined the bile of dogs before and after the injection under varying conditions of sheep's bile. The characteristic absorption spectrum of cholohæmatine, a colouring matter not present in the bile of the dog, but always a constituent of sheep's bile, was invariably found in bile secreted by the dog's liver after injection; thus an indisputable proof is given that the liver takes out bile constituents from the blood, and passes them into the alimentary canal unaltered.

BRUSSELS.

Academy of Sciences, July 4.—M. Plateau in the chair.—On hoar frosts, by M. Folie. Some observations of the ravages caused by frosts which occurred on June 12 and 13 indicate that, if the cultures of the Ardennes are to be preserved from such disastrous effects, the plateau must be again planted with trees. The frosts appear to have had more effect near the soil than at some metres above it.—On one of M. Servais's theorems, by M. E. Catalan.—On an extension of M. Hermite's law of reciprocity, by M. Jacques Deruyts.—On two new Lerneopodians, one of which is found at the Azores, and the other on the coast of Senegal, by M. P. J. Van Beneden. Description is given of male and female *Brachiella chavuii* found at the Azores, and of male and female *Brachiella chevreuxii* from the coast of Senegal. The description is accompanied by a plate.—On a method of generation of the cubic surface, by M. F. Deruyts.

SYDNEY.

Royal Society of New South Wales, July 1.—H. C. Russell, F.R.S., President, in the chair.—Eighteen new members were elected, and the following papers were read:—On Nos. 13 and 14 compressed-air flying machines, by Lawrence Hargrave.—Some folk-songs and myths from Samoa, translated by the Rev. G. Pratt, with introductions and notes by Dr. John Fraser.—On a cyclonic storm in the Gwydir district, and Preparations now being made in Sydney Observatory for the photographic chart of the heavens (illustrated by photographs), by H. C. Russell, F.R.S., Government Astronomer.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

British Cicadæ, Part 7: G. B. Buckton (Macmillan).—Bibliotheca Botanica (Wesley).—British Oligocene and Eocene Mollusca in the British Museum (Natural History): R. B. Newton (London).—Fossil Botany: H. Graf zu Solms-Laubach; translated by H. E. F. Garnsey, revised by I. B. Balfour (Oxford, Clarendon Press).—Synopsis der Hoeheren Mathematik, Erster Band: J. G. Hagen (Berlin, Dames).—Missouri Botanical Garden Second Annual Report (St. Louis, Mo.).—Blackie's Science Readers, Nos. 2, 4, and 5 (Blackie).—Free Land: Dr. T. Hertzka, translated by A. Ransom (Chatto and Windus).—A Sketch of the Vegetation of British Baluchistan: J. H. Lace and W. B. Hemsley (London).—Bulletins de la Société d'Anthropologie de Paris, January and February, March and April (Paris, Masson).—Papers and Proceedings of the Royal Society of Tasmania for 1890 (Hobart).

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